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ART. I.—*An account of the Meteor which burst over Weston in Connecticut, in December*, 1807, *and of the falling of stones on that occasion;* by Professors SILLIMAN and KINGSLEY.*

ON the 14th of December, 1807, about half past 6 o'clock, A. M., a meteor was seen moving through the atmosphere, with very great velocity, and was heard to explode over the town of Weston, in Connecticut, about 25 miles west of New Haven. Nathan Wheeler, Esq., of Weston, one of the justices of the court of common pleas for the county of Fairfield, a gentleman of great respectability, and of undoubted veracity, who seems to have been entirely uninfluenced by fear or imagination, was passing at the time through an inclosure adjoining his house, and had an opportunity of witnessing the whole phenomenon. From him the account of the appearance, progress, and explosion of the meteor, is principally derived. The morning was somewhat cloudy. The clouds were dispersed in unequal masses, being in some places thick and opaque, and in others fleecy and partially transparent. Numerous spots of unclouded sky were visible, and along the northern part of the horizon a space of ten or fifteen degrees was perfectly clear. The attention of Judge Wheeler was first drawn by a sudden flash of light, which illuminated every object. Looking up he discovered in the north a globe of fire, just then passing behind the cloud, which obscured, though it did not entirely hide the meteor. In this

* No detailed account of the Weston Meteor has ever appeared in this Journal. It has been thought proper therefore to reproduce the following account from the Memoirs of the Connecticut Academy of Arts and Sciences. This is introductory to some considerations upon the path of the body.

situation its appearance was distinct, and well defined, like that of the sun seen through a mist. It rose from the north, and proceeded in a direction nearly perpendicular to the horizon, but inclining, by a very small angle, to the west, and deviating a little from the plane of a great circle, but in pretty large curves, sometimes on one side of the plane, and sometimes on the other, but never making an angle with it of more than 4 or 5 degrees. Its apparent diameter was about one-half or two-thirds the apparent diameter of the full moon. Its progress was not so rapid as that of common meteors and shooting stars. When it passed behind the thinner clouds, it appeared brighter than before; and when it passed the spots of clear sky, it flashed with a vivid light, yet not so intense as the lightning in a thunder storm, but rather like what is commonly called *heat lightning*.

Where it was not too much obscured by thick clouds, a waving conical train of paler light was seen to attend it, in length about 10 or 12 diameters of the body. In the clear sky a brisk scintillation was observed about the body of the meteor, like that of a burning firebrand carried against the wind.

It disappeared about 15 degrees short of the zenith, and about the same number of degrees west of the meridian. It did not vanish instantaneously, but grew, pretty rapidly, fainter and fainter, as a red hot cannon-ball would do, if cooling in the dark, only with much more rapidity.

There was no peculiar smell in the atmosphere, nor were any luminous masses seen to separate from the body. The whole period between its first appearance and total extinction, was estimated at about thirty seconds.

About thirty or forty seconds after this, three loud and distinct reports, like those of a four-pounder, near at hand, were heard. They succeeded each other with as much rapidity as was consistent with distinctness, and, altogether, did not occupy three seconds. Then followed a rapid succession of reports less loud, and running into each other, so as to produce a continued rumbling, like that of a cannon-ball rolling over a floor, sometimes louder, and at other times fainter: some compared it to the noise of a wagon, running rapidly down a long and stony hill; or, to a volley of musketry, protracted into what is called, in military language, *a running fire*. This noise continued about as long as the body was in rising, and died away apparently in the direction from which the meteor came.

The accounts of others corresponded substantially with this. Time was differently estimated by different people. Some augmented the number of loud reports, and terror and imagination seem, in various instances, to have magnified every circumstance of the phenomenon.

The only thing which seemed of any importance beyond this statement, was derived from Mr. Elihu Staples, who said, that when the meteor disappeared, there were apparently three successive efforts or leaps of the fire-ball, which grew more dim at every throe, and disappeared with the last.

The meteor was seen as far south as New York; and the explosion was heard, and a tremulous motion of the earth perceived, between forty and fifty miles north of Weston. From the various accounts which we have received of the appearance of this body at different places, we are inclined to believe, that the time between the disappearance and report, as estimated by Judge Wheeler, is too little, and that a minute is the least time which could have intervened. Taking this, therefore, for the time, and the apparent diameter of the body as only half that of the full moon, its real diameter could not be much less than 300 feet.*

We now proceed to detail the consequences which followed the explosion and apparent extinction of this luminary.

We allude to the fall of a number of masses of stone in several places, principally within the town of Weston. The places which had been well ascertained at the period of our investigation were six. The most remote were about 9 or 10 miles distant from each other, in a line differing little from the course of the meteor. It is therefore probable that the successive masses fell in this order, the most northerly first, and the most southerly last. We think we are able to point out three principal places where stones have fallen, corresponding with the three loud cannon-like reports, and with the three leaps of the meteor, observed by Mr. Staples. There were some circumstances common to all the cases. There was in every instance, immediately after the explosions had ceased, a loud whizzing or roaring noise in the air, observed at all the places, and so far as was ascertained, at the moment of the fall. It excited in some the idea of a tornado; in others, of a large cannon-shot in rapid motion, and it filled all with astonishment and apprehension of some impending catastrophe. In every instance, immediately after this, was heard a sudden and abrupt noise, like that of a ponderous body

* From subsequent information it appears that this meteor was seen in the eastern part of Connecticut, in New Jersey, in the interior of the state of New York, and as high up, at least, as Rutland, in Vermont.

It was stated by Professor Day, in a discourse before the Connecticut Academy, that a gentleman who was riding in Colchester in Connecticut, which is about 50 miles east of Weston, saw this meteor distinctly; it was passing within 15 or 20 degrees of the moon, and appeared to him to be about one-half as large as that luminary. It was justly remarked by Mr. Day that, if at this distance, it had this apparent diameter, its real diameter must have been 1200 or 1300 feet, or, about a quarter of a mile; but, as the apparent diameter was not taken with an instrument, but by estimation, it was not supposed that this conclusion was perfectly exact. It is evident, at least, that the meteor must have been much higher, when it exploded, than was at first supposed.

striking the ground in its fall. Excepting one, the stones were more or less broken. The most important circumstances of the particular cases were as follows :

I. The most northerly fall was within the limits of Huntington, on the border of Weston, about 40 or 50 rods east of the great road from Bridgeport to Newtown, in a cross road, and contiguous to the house of Mr. Edwin Burr. Mr. Burr was standing in the road, in front of his house, when the stone fell. The noise produced by its collision with a rock of granite was very loud. Mr. Burr was within 50 feet, and immediately searched for the body, but it being still dark, he did not find it till half an hour after. By the fall, some of it was reduced to powder, and the rest of it was broken into very small fragments, which were thrown around to the distance of 20 or 30 feet. The rock was stained at the place of contact with a deep lead color. The largest fragment which remained did not exceed the size of a goose egg, and this Mr. Burr found to be still warm to his hand. There was reason to conclude from all the circumstances, that this stone must have weighed about twenty or twenty-five pounds.

Mr. Burr had a strong impression that another stone fell in an adjoining field, and it was confidently believed that a large mass had fallen into a neighboring swamp, but neither of these had been found. It is probable that the stone, whose fall has now been described, together with any other masses, which may have fallen at the same time, was thrown from the meteor at the first explosion.

II. The masses, projected at the second explosion, seem to have fallen principally at and in the vicinity of Mr. William Prince's in Weston, distant about five miles, in a southerly direction, from Mr. Burr's. Mr. Prince and family were still in bed, when *they heard a noise like the fall of a very heavy body, immediately after the explosions.* They formed various unsatisfactory conjectures concerning the cause, nor did even a fresh hole made through the turf in the door yard, about 25 feet from the house, lead to any conception of the real cause.

They had indeed formed a vague conjecture that the hole might have been made by lightning, but would probably have paid no further attention to the circumstance, had they not heard, in the course of the day, that stones had fallen that morning in other parts of the town. This induced them, towards evening, to search the hole in the yard, where they found a stone buried in the loose earth which had fallen in upon it. It was two feet from the surface, the hole was about twelve inches in diameter, and as the earth was soft and nearly free from stones, the mass had sustained little injury, only a few small

fragments having been detached by the shock. The weight of this stone was about thirty-five pounds. From the descriptions which we have heard, it must have been a noble specimen, and men of science will not cease to deplore that so rare a treasure should have been immediately broken in pieces. All that remained unbroken of this mass, was a piece of twelve pounds weight, since purchased by Isaac Bronson, Esq., of Greenfield, with the liberal view of presenting it to some public institution.

Six days after, another mass was discovered, half a mile northwest from Mr. Prince's. The search was induced by the confident persuasion of the neighbors that they heard it fall near the spot, where it was actually found buried in the earth, weighing from seven to ten pounds. It was found by Gideon Hall and Isaac Fairchild. It was in small fragments, having fallen on a globular detached mass of gneiss rock, which it split in two, and by which it was itself shivered to pieces.

The same men informed us, that they suspected another stone had fallen in the vicinity, as the report had been distinctly heard, and could be referred to a particular region somewhat to the east. Returning to the place after an excursion of a few hours to another part of the town, we were gratified to find the conjecture verified, by the actual discovery of a mass of thirteen pounds weight, which had fallen half a mile to the northeast of Mr. Prince's. Having fallen in a ploughed field, without coming into contact with a rock, it was broken only into two principal pieces, one of which, possessing all the characters of the stone in a remarkable degree, we purchased; for it had now become an article of sale.

Two miles southeast from Mr. Prince's, at the foot of Tashowa Hill, a fifth mass fell. Its fall was distinctly heard by Mr. Ephraim Porter and his family, who live within forty rods of the place, and in full view. They saw a smoke rise from the spot, as they did also from the hill, where they are positive that another stone struck, as they heard it distinctly. At the time of the fall, having never heard of any such thing, they supposed that lightning had struck the ground, but after three or four days, hearing of the stones which had been found in their vicinity, they were induced to search, and the result was the discovery of a mass of stone in the road, at the place where they supposed the lightning had struck. It penetrated the ground to the depth of two feet in the deepest place; the hole was about twenty inches in diameter, and its margin was colored blue from the powder of the stone, struck off in its fall. It was broken into fragments of moderate size, and from the best calculations might have weighed 20 or 25 pounds.

The hole exhibited marks of much violence, the turf being very much torn, and thrown about to some distance.

We searched several hours for the stone, which was heard to fall on the hill, but without success. Since that time, however, it has been discovered. It is unbroken, and exactly corresponds in appearance with the other specimens. It weighs 36½ pounds.* It is probable that the five stones last described were all projected at the second explosion.

III. At the third explosion a mass of stone far exceeding the united weight of all we have hitherto described, fell in a field belonging to Mr. Elijah Seeley, and within thirty rods of his house. Mr. Seeley's is at the distance of about four miles from Mr. Prince's. Mr. Elihu Staples lives on the hill, at the bottom of which this body fell, and carefully observed the whole phenomenon.

After the last explosion, he says, a rending noise like that of a whirlwind passed along to the east of his house and immediately over his orchard, which is on the declivity of the hill. At the same instant a streak of light passed over the orchard in a large curve, and seemed to pierce the ground. A shock was felt, and a report heard like that of a heavy body falling to the earth; but no conception being entertained of the real cause (for no one in this vicinity, with whom we have conversed, appeared to have ever heard of the fall of stones from the skies), it was supposed that lightning had struck the ground. Three or four hours after the event, Mr. Seely went into his field to look after his cattle. He found that some of them had leaped into the adjoining inclosure, and all exhibited strong indications of terror. Passing on, he was struck with surprise at seeing a spot of ground which he knew to have been recently turfed over, all torn up, and the earth looking fresh, as if from recent violence. Coming to the place, he found a great mass of fragments of a strange looking stone, and immediately called for his wife, who was second on the ground.

Here were exhibited the most striking proofs of violent collision. A ridge of micaceous schistus lying nearly even with the ground, and somewhat inclining like the hill to the southeast, was shivered to pieces, to a certain extent, by the impulse of the stone, which thus received a still more oblique direction, and forced itself into the earth to the depth of three feet, tearing a hole of five feet in length and four and a half feet in breadth, and throwing large masses of turf and fragments of

* It has been purchased by Mr. Gibbs, of Newport, Rhode-Island, who has thus enriched his splendid collection of minerals with the finest meteoric stone which is probably extant. This specimen abounds so much in iron, that it might almost be denominated an iron ore; some of the pieces of iron visible on the surface are more than an inch long.

stone and earth to the distance of 50 and 100 feet. Had there been no meteor, no explosions, and no witnesses of the light and shock, it would have been impossible for any person contemplating the scene to doubt, that a large and heavy body had really fallen from the skies with tremendous momentum.

From the best information which we could obtain of the quantity of fragments of this last stone, compared with its specific gravity, we concluded that its weight could not have fallen much short of 200 pounds. All the stones, when first found, were friable, being easily broken between the fingers; this was especially the case, where they had been buried in the moist earth; but by exposure to the air, they gradually hardened.

This stone was all in fragments, none of which exceeded the size of a man's fist, and was rapidly dispersed by numerous visitors, who carried it away at pleasure. Indeed we found it difficult to obtain a sufficient supply of specimens of the various stones, an object, which was at length accomplished, principally by importunity and purchase.

The specimens obtained from the different places are perfectly similar. The most superficial observer would instantly pronounce them portions of a common mass. Few of the specimens weigh one pound, most of them less than half a pound, and from that to the fraction of an ounce.

The piece lately found on Tashowa Hill is the largest with which we are acquainted. Mr. Bronson's is the next in size. The largest specimen in our possession weighs six pounds, and is very perfect in its characteristic marks. Of smaller pieces we have a good collection. They possess every variety of form, which might be supposed to arise from fracture with violent force. On many of them, and chiefly on the large specimens, may be distinctly perceived portions of the external part of the meteor. It is everywhere covered with a thin black crust, destitute of splendor, and bounded by portions of the large irregular curve, which seems to have enclosed the meteoric mass. This curve is far from being uniform. It is sometimes depressed with concavities, such as might be produced by pressing a soft and yielding substance. The surface of the crust feels harsh, like the prepared fish skin, or shagreen. It gives sparks with the steel. There are certain portions of the stone covered with the black crust, which appear not to have formed a part of the outside of the meteor; but to have received this coating in the interior parts, in consequence of fissures or cracks, produced probably by the intense heat, to which the body seems to have been subjected. These portions are very uneven, being full of little protuberances. The specific gravity of the stone is 3·6,

water being one. The specific gravity of different pieces varies a little; this is the mean of three.

The color of the mass of the stone is mainly dark ash, or, more properly, a leaden color. It is interspersed with distinct masses, from the size of a pin's head to the diameter of one or two inches, which are almost white, resembling in many instances, the crystals of feldspar in some varieties of granite. The texture of the stone is granular and coarse, resembling some pieces of grit stone. It cannot be broken by the fingers, but gives a rough and irregular fracture with the hammer, to which it readily yields. On inspecting the mass, five distinct kinds of matter may be perceived by the eye.

1. The stone is thickly interspersed with black or grey globular masses, most of them spherical, but some are oblong. Some of them are of the size of a pigeon shot, and even of a pea, but generally they are much smaller. They can be detached by any pointed iron instrument, and leave a concavity in the stone. They are not attractable by the magnet, and can be broken by the hammer. If any of them appear to be affected by the magnet, it will be found to be owing to the adherence of a portion of metallic iron.

2. Masses of yellow pyrites may be observed. Some of them are of a brilliant golden color, and are readily distinguishable by the eye. Some are reddish and some are whitish. The pyrites appear most abundant in the light colored spots, where they exhibit very numerous and brilliant points, which are very conspicuous through a lens.

3. The whole stone is interspersed with malleable iron, alloyed with nickel. These masses of malleable iron are very various in size, from mere points to the diameter of half an inch. They may be made very conspicuous by drawing a file across the stone.

4. The lead-colored mass has been described already, and constitutes by far the greater part of the stone. After being wet and exposed to the air, the stone becomes covered with numerous reddish spots, which do not appear in a fresh fracture, and arise manifestly from the rusting of the iron.

5. There are a few instances of matter dispersed irregularly through the stone, which are considered as intermediate between pyrites and malleable iron. They are sometimes in masses apparently crystalline, but usually irregular. They are black, and commonly destitute of splendor, but exposed by a recent fracture, they appear like a glossy superficial coating. They are sometimes attractable by the magnet, and sometimes not.

ART. II.—*On the distillation of dense Hydrocarbons at high temperatures, technically termed "Cracking;"* by S. F. PECKHAM.

IN the American reprint of the Chemical News, for June of this year, an article appears "On Naphtha and Illuminating Oil from heavy California Tar," by Prof. B. Silliman, copied from the San Francisco Bulletin. In the September number of the same journal, an article appears "On the Distillation of Hydrocarbons," by Joseph Hirsch, Ph.D., in which the results obtained by Prof. S. are subjected to criticism, and certain statements made in reference to the subject of a most extraordinary character.

At the same time that Mr. Corning was engaged upon the experiments, the results of which form the subject of Prof. Silliman's paper, I was engaged upon experiments of a similar character for the Geological Survey of California, the results of which have not yet been published.* These results differed somewhat from those obtained by Prof. S., as also the method by which they were obtained. I shall, therefore, give a brief summary, both of method and results, and compare the conclusions to be derived from them with the statements made by Dr. Hirsch.

Those who first attempted the manufacture of commercial oils from crude California materials, when operating with the upright still in common use for the manufacture of Pennsylvania oils, encountered an apparently insurmountable obstacle, viz: a large proportion of the distillate was neither light nor heavy, neither burning oil nor lubricating oil, but an oil intermediate in density between the two, and therefore not merchantable. The difficulty was so far overcome by enclosing the stills in brick-work, heating them *entirely* by radiant heat, and distilling very slowly, that the amount of heavy lubricating oil was largely increased, and the "middlings" correspondingly diminished. The yield of illuminating oil, however, was very slightly increased, and it was for the purpose of securing a larger yield of that material, that my experiments were undertaken. I had at first intended to subject them to Mr. Downer's process of slow distillation in a high, upright still, the top of which was exposed to radiation. The small quantity of crude material at my command (5 gallons of each variety) rendered this operation exceedingly difficult to conduct suc-

* The volume of Reports of the Survey on "Economical Geology," containing these results, is now ready for the press, but its publication is delayed by the failure of the last California Legislature in making the necessary appropriation.

cessfully, and it was with extreme satisfaction that I saw at the time, in the October number of the Chemical News for 1866, an announcement that Mr. Young of Glasgow had obtained a patent for the manufacture of illuminating oils from heavy paraffine oils, by distillation under pressure. It was necessary that I should operate on a small quantity at a time, and also, that I should subject the four or five different samples which I had, to the same treatment, in order that I might compare the results, and judge of their relative value. For that purpose I contrived the apparatus described in the September number of this Journal for 1867, which, so far as I know, has but one fault, viz: the chamber of the valve is too small, and should be enlarged sufficiently to enable the pressure to be regulated by weights instead of by a spring.

With a pressure of between 30 and 40 pounds per square inch (the exact amount was not ascertained) the following results were obtained. See table, page 11.

No. I was an oil procured from one of the tunnels of the Hayward Petroleum Company, of a specific gravity of ·9023, yielding by distillation in a common still about 15 per cent of light oil, of a specific gravity of ·810, with about 40 to 50 per cent of "middlings," and 20 per cent of light lubricating oil.

No. II came from the celebrated Pico Spring, yielding the lightest oil of any natural outcrop in southern California. Its specific gravity was ·8932, and it yielded to treatment by the ordinary method only about 20 per cent of illuminating oil of the proper density.

No. III was from the Cañada Laga, of a specific gravity of ·9184, and yielding by the ordinary process only 3 per cent of illuminating oil.

No. IV was a sample of Maltha from the same spring as that operated upon by Prof. Silliman. Its specific gravity was ·978, and it yielded about 2·5 per cent of illuminating oil.

This table exhibits the results of actual experiment, not of theory, and while they differ from those obtained by Prof. Silliman, the difference is in degree and not in kind, and is without doubt due to the superiority of the apparatus used by myself, and to the higher degree of pressure employed. Both series of experiments confirm each other, and alike prove, that dense petroleums and a thick heavy tar—as thick as ordinary molasses—which yield practically little or no illuminating oil by ordinary treatment, by distillation under pressure are subjected to what is technically termed "cracking," and made to yield from 28 to 60 per cent of oil fit for burning, and rendered thereby nearly as valuable as the crude oils of Pennsylvania.

Percentage results of Distillation under Pressure.

Number	I.	II.	III.	IV.
Volatile impurity, consisting of air and water,	----	----	----	12·5
1st. Pressure Distillation of crude material,	91·—	87·66	82·86	72·20
Coke and loss at do.,	9·—	12·34	17·34	15·50
1st Fractionation of sp. gr. 43° B. = ·810,	42·—	56·72	40·33	16·70
Leaving Heavy Residue for re-distillation,	49·—	30·94	42·33	55·30
Which yielded, by 2d Pressure Distillation,	44·15	27·84	58·09	49·80
2d. Fractionation of sp. gr. 43° B. = ·810,	12·25	6·96	9·52	12·40
Total crude Illuminating Oil,	54·25	62·68	49·85	29·10
Loss in treatment of do., $\frac{3}{100}$,	1·62	1·88	1·49	·90
Total yield of Refined Oil,	51·25	60·70	48·36	28·20
Total crude Lubricating Oil,	31·85	20·88	28·57	37·40
Loss in treatment of do., $\frac{3}{100}$,	·95	·62	·85	1·10
Total Refined Lubricating Oil,	30·90	20·26	27·72	36·30
Yield of Refined Illuminating Oil, sp. gr. 43° B.,	51·25	60·70	48·36	28·20
Yield of Refined Lubricating Oil, sp. gr. 23°–25° B.,	30·90	20·26	27·72	36·30
Loss in treatment,	2·61	2·50	2·34	2·00
Coke and loss in Distillation,	18·24	16·54	21·58	33·50
Yield of Illuminating Oil by ordinary process,	15·00	20·00	3·00	2·50

Dr. Hirsch criticises these experiments as not being executed under circumstances "parallel to the distillation on a large scale." He then states, that during the distillation of hydrocarbons on the large scale, "the process of 'cracking' always takes place in some degree," as "all hydrocarbons of high boiling points contained in such mixtures are, during distillation, exposed to varying degrees of temperature below their own boiling point, as long as those hydrocarbons of lesser gravity and lower boiling point have not been removed by distillation." He further states, that "it is this exposure to a lower degree of heat than corresponds to the distilling point of an oil of definite gravity, which comprises the operation of 'cracking.'" Again he states, that during slow distillation in the enormous stills now being introduced, "cracking" takes place without any "special efforts" to produce such a result, "while only a small portion distills over as paraffine oil, that being due to over-heating." He next states, that by rapid distillation of a small quantity, the different hydrocarbons which make up the petroleum come over unchanged, and that the difference between this last named distillation and the former, is the same as the one between distilling coal for the production of illuminating gas, and that for producing coal oil; the former producing a dense tar, being carried on in small low retorts, and the latter, in revolving retorts of large capacity. "In these the oily vapors are exposed to a cooler temperature than their own with every revolution of the retort, and are in this manner broken up into oils of lighter gravity."

He then gives a number of rules, the result of his own experience.

"1st. * * * the more the temperature of the actual boiling point of oil of definite gravity is above the temperature to which the same oil is raised, the greater is the quantity of light oil obtained.

"2d. The gravity of distillate resulting from reduction of temperature will be directly proportionate to said reduction. * * * In distillation, the temperature, therefore, should always be reduced to the boiling point of the oil of the specific gravity desired.

"3d. The difference between the temperatures of the two boiling points, viz: of the oil being subjected to distillation, and of the derived distillate, is in direct proportion to the height of the still employed, or to the facility for cooling the upper portions of the still.

"4th. The intensity of the process of 'cracking' is proportionate to the suddenness with which the oil vapors are condensed before leaving the still.

"5th. The difference in gravity between that of the oil distilled, and the desired distillate, is in direct proportion to the quantity of water produced in the process.

"These laws are the same with hydrocarbons distilled under the ordinary atmospheric pressure, as with those distilled in a vacuum, or under increased pressure."

It is very rarely that as many errors are included within as little space, and the entire discussion exhibits in a remarkable degree to what totally erroneous conclusions the results of close observation and experience may lead, when explained upon a false hypothesis.

The operation of "cracking," as conducted by Mr. Downer, consists in a slow distillation of oils of high specific gravity, and high boiling point, in a still furnished with a high dome, the outer surface of which is freely exposed to radiation. As distillation proceeds, those oils which are condensed at the temperature at which the dome is maintained, instead of passing into the worm and thence into the receiver, collect in drops upon the surface of the dome, and fall back upon the surface of the oil beneath, which has meantime become heated above their boiling points. Mr. Young distills the oils under pressure, thereby vaporizing them at a temperature above their normal boiling points.

It is therefore obvious, that the primary and essential condition of "cracking" is simply to subject the oils to a temperature above their boiling points, or in other words, to super-heat their vapors. It will be found that for oils of the same density,

the higher the temperature to which they are raised, or at which they are distilled, the lighter will be the product; and that to produce an oil of given density, the heavier oil must be raised to a certain fixed temperature, the intensity of heat depending on the lightness of the oil required.

Now it is evident that Prof. Silliman could not subject five or ten gallons of Maltha to experiment *strictly* analogous to Mr. Downer's process, the two elements of time and large capacity of apparatus being practically unattainable when manipulating so small a quantity. He could, however, follow Mr. Young's process *strictly*, using from 10 to 15 pounds pressure per square inch. My own results were obtained by using from 30 to 40 pounds pressure per square inch, and operating upon only 1,500 cubic centimeters at a time.

Dr. Hirsch is correct in stating that during the distillation on the large scale, this process always obtains action in some degree, but his reasoning is utterly at fault. So, too, is his explanation of the fact, that "cracking" takes place in large stills without any special effort to secure such a result. The real explanation lies in the fact, that the upper portion of stills in ordinary use is generally exposed to atmospheric currents and radiation. With such an arrangement it is impossible, upon Mr. Downer's plan, to prevent more or less condensation upon the dome, and consequent "cracking," especially toward the end of the operation, in stills of the enormous capacity of 40,000 gallons, where all the conditions essential to his process are present. It was in stills set in this manner that the heavy California oils were first distilled, and in which they were "cracked" to an oil of medium density; but when the sides and domes of the stills were surrounded with brick-work, the vapors were no longer condensed, and they passed unchanged into the receiver.

Dr. Hirsch is again in error, in supposing that paraffine oils are produced by a high temperature. I am told that Mr. Downer has made illuminating oils, by "cracking" solid paraffine wax by means of his process. The paraffine lubricating oils of commerce are now most successfully produced from coals, by distilling the material in large kilns, in which combustion takes place at the upper surface, and the draft is conducted downward, insuring the expulsion of the volatile products at the very lowest temperature possible.

He is yet again in error, in the analogy which he assumes to exist between rapid and slow distillation of petroleum, and the distillation of coals, in small retorts to produce illuminating gas, and in revolving retorts to produce oil. Rapid distillation "cracks" the oil, because it necessitates increased

temperature to force the vapors from the still. Such has been my experience repeatedly on both the large and small scale. Slow distillation yields the hydrocarbons *unchanged*, provided the vapors have ready egress from the still, because distillation is then carried on at the lowest temperature attainable. Small retorts are used for the manufacture of gas, in order that the coal may the sooner be raised to the red-heat, and the greatest possible "cracking" effect be experienced, while revolving retorts are used in the manufacture of oil, not that the charge may be repeatedly cooled, but in order that it may be uniformly heated, avoiding the necessity of over-heating the portion next the fire, in order that the upper portion may be heated sufficiently.

His first and second rules, when reduced to plain English, assert that "cracking" may be produced by refrigeration. Following their lead, in order to produce from paraffine wax the lightest member of the naphtha series isolated by Prof. Warren, and boiling at 0° C., the paraffine should be immersed in melting ice. According to these rules, the best method of producing illuminating gas from crude petroleum would be, to subject the oils to the action of a refrigerating mixture of solid carbonic acid and ether, instead of allowing them to drip upon red-hot coke.

His third rule is correct, as the lower the temperature at which the top of the still is maintained, the lower will be the boiling point of the liquid resulting from the condensation of the vapors that escape.

His fourth rule is too obscure in its signification to admit of criticism.

His fifth rule is of the most extraordinary character. Chemistry is not yet ready for the announcement of the transmutation of one element into another, and such transmutation must certainly take place if WATER *can* be produced by distillation of volatile HYDROCARBONS, with exclusion of OXYGEN. So, too, is it almost equally difficult to imagine how any general laws can be "the same" for two processes so diametrically opposed as distillation in a vacuum and under pressure.

I desire in this connection to note a few suggestions which have occurred to me in reference to this subject. In the last edition of Prof. Dana's Mineralogy (1868), he has classified the results obtained by Profs. Warren and Storer, and arranged the hydrocarbons isolated by them in three groups, viz: the Naphtha and Beta-naphtha series, and the Pittoleum group. The first two are isomeric, the last contains more carbon in proportion to its hydrogen. The members of the Pittoleum group at present isolated are doubtless the lower members of a

large group, the higher members of which have very high boiling points; or perhaps there is still another group containing a still larger proportion of carbon. As the different members of these groups decrease in density, the proportion of hydrogen increases, and as they increase in density, the proportion of carbon increases. The process of "cracking" Pennsylvania oils, therefore, is simply subtraction of carbon; and it appears from the results of experiment and analysis, that each additional atom of carbon is held by a feebler affinity than the last, consequently the stability of the members increases as the proportion of carbon decreases. The lower the member is in the series, the stronger is the affinity of the hydrogen for the carbon, and consequently, the higher is the temperature required for the production of the member next below it. Thus it is that over-heating dense paraffine oils produces medium or illuminating oils; over-heating illuminating oils produces still lower and more volatile liquids; at a still higher temperature the products become gaseous, and at an excessively high temperature, light rather than heavy carburetted hydrogen gas is produced.

In the absence of actual demonstration by fractionation, I am led to believe from the behavior of California petroleums, that they do not contain either the Naphtha or Beta-naphtha series in appreciable quantity; nor do they contain the members of the Pittoleum group yet isolated in large proportion, but are doubtless made up of the higher members of that group, or a still more highly carbonized and unstable group not yet described, with which is mingled one or more nitro-hydrocarbons yet more easily decomposed. Be this fact or fancy, the appearance and physical properties of the refined pressure distillate from these oils, lead to the opinion that it is made up of the same members of which refined Pennsylvania petroleum is composed. The lightest oils existing in crude California petroleum change in a few weeks, after treatment, to a dirty yellow color, even when tightly corked and exposed only to the light. A bottle of refined pressure distillate in my possession has now been prepared nearly two years, yet its color has scarcely changed perceptibly.

By Prof. Warren's process of fractionation only a trace was eliminated from any of my samples of crude California oil under 150° C., yet in one instance, when my valve accidentally stuck so that the pressure was very considerably increased above 40 pounds, the vapors when they escaped passed through the worm uncondensed at 8° C., and melted the lead pipe at its point of connection with the retort; proving that as in the case of the heavy paraffine oils, decrease in the density of the

distillate follows any considerable increase in the temperature of distillation.

I hope at some future day to be able to fractionate both the crude California petroleums, and the products of their distillation under pressure, and thus obtain some additional facts in reference to this interesting question.

Cambridge, Mass., Nov. 9th, 1868.

ART. III.—*On the Chromites of Magnesium;* by W. R. NICHOLS, student at the Massachusetts Institute of Technology.

THE extent of the power possessed by some of the hydrated sesquioxyds, notably by the hydrate of chromium, to hold magnesia and several other of the metallic protoxyds in insoluble combination, appears never to have been distinctly recognized by chemists. Fresenius, it is true, remarks* that sesquioxyd of chromium cannot be separated (quantitatively) by ammonia from the alkaline earths, since, even though carbonic acid be completely excluded, portions of the alkaline earths are thrown down in combination with the sesquioxyd of chromium. But his statement is far less forcible than the facts in the case demand.

Other chemists have fallen into the grave error of supposing that magnesium can be separated from chromium, with sufficient accuracy for the ordinary purposes of qualitative analysis, by mere addition of ammonia-water to throw down hydrate of chromium, after the solution to be analyzed has been mixed with chlorid of ammonium. Galloway† has developed a plan for separating the hydrates of iron, aluminum and chromium as a distinct group or class, by adding chlorid of ammonium and ammonia to the boiled filtrate from the precipitate produced by sulphuretted hydrogen in the ordinary course of an analysis. According to Galloway, this ammonia precipitate is collected by itself and examined for iron, aluminum and chromium; sulphid of ammonium is then added to the filtrate to throw down cobalt, nickel, zinc and manganese; carbonate of ammonium is next employed to precipitate barium, strontium and calcium, while the magnesium, if any be present, is finally detected by means of phosphate of sodium in the usual way.

As will appear, however, from the following experiments, it would be quite impossible in many cases to detect magnesium in this way in presence of chromium. From a solution con-

* Quantitative Chemical Analysis, 4th ed. London, 1865, p. 372.

† Manual of Qualitative Analysis, 4th ed. London, 1864, p. 84 et seq.

taining any considerable proportion of sesquioxyd of chromium as well as magnesium, the greater part or the whole of the latter will be thrown down, together with the chromium, on the addition of ammonia, even when the solution is highly charged with chlorid of ammonium.

Sulphid of ammonium, also, like ammonia, throws down from mixed solutions of chromium and magnesium a compound precipitate well-nigh insoluble in water and saline solutions. If no serious difficulty has been met with hitherto in detecting magnesium in presence of chromium by the ordinary method of qualitative analysis, it is doubtless owing to the fact that, in pursuing this method, some of the magnesium thrown down in combination with chromium will be encountered when the precipitate produced by sulphid of ammonium comes to be examined in regular course for (barium, strontium, calcium and) magnesium, which may have fallen down in combination with phosphoric, boracic, oxalic, or silicic acid, or in the form of fluorids; and this, even if nothing but chromium and magnesium are present.

As thrown down either by ammonia or by sulphid of ammonium, the compound precipitate of chromium and magnesium is nearly or quite insoluble in solutions of the fixed caustic alkalies, and even when the precipitate is dissolved in chlorhydric acid and the solution mixed with cold soda lye, all the magnesium and most of the chromium is re-precipitated. Since this behavior of the precipitate toward soda lye points clearly to the existence of chemical attraction or affinity between the magnesia and the chromic oxyd, I have devoted particular attention to the study of the precipitates thus produced by soda.

Equivalent quantities of chrome alum and Epsom salt were weighed out and dissolved in water with addition of chlorid of ammonium and the mixture was treated with ammonia-water in slight excess. It was found to make no difference with regard to the completeness of the precipitation whether the salts were dissolved together with addition of chlorid of ammonium, or separately, the chlorid of ammonium being added to the solution of sulphate of magnesium before mixing the two solutions.

After the addition of ammonia to the mixed solution of chrome alum and sulphate of magnesium, an attempt was made in each case to determine the amount of magnesium in the filtrate by precipitation with phosphate of sodium. Negative results were obtained for the most part in these trials, as will be seen below.

The precipitate thrown down by ammonia was dissolved in

boiling dilute chlorhydric acid, the solution thoroughly cooled, and then mixed with an excess of a cold aqueous solution of caustic soda. The precipitate produced by the soda was filtered as rapidly as possible, washed somewhat with water, dried, ignited and weighed, while the filtrate was boiled to precipitate any chromium which had dissolved. The hydrate of chromium thrown down by boiling was collected on a filter, was washed, dried, ignited and weighed by itself.

I. 2·5098 grm. of chrome alum and 0·5240 grm. of Epsom salt were taken, so that the mixture contained 1·19 equivalent of Cr_2O_3 and 1 equivalent of MgO. No magnesium could be detected in the filtrate from the precipitate produced by ammonia in this mixture. The ammonia precipitate dissolved in chlorhydric acid, and treated with caustic soda in excess, gave up only 0·0103 grm. of Cr_2O_3 to the soda, while 0·3753 grm. of Cr_2O_3 went down again with the magnesium.

An analysis of the ignited soda precipitate gave the following results :—

		Taken.	Found.		Theory (MgO, Cr_2O_3).
(I.)	MgO	0·0847	0·0831	18·13	20·73
	Cr_2O_3	0·3843	0·3753	81·87	79·27
		0·4690	0·4584	100·00	100·00

II. 2·4979 grm. of chrome alum and 0·6150 grm. of Epsom salt were taken, so that the mixture contained 1 equivalent of Cr_2O_3 and 1 equivalent of MgO. Sulphid of ammonium was used as the precipitant in this instance, and 0·0105 grm. of MgO was found in the filtrate from it. When the precipitate was dissolved in chlorhydric acid and treated with soda lye, 0·0181 grm. of Cr_2O_3 went into solution in the soda.

III. 2·469 grm. of chrome alum and 0·302 grm. of Epsom salt were taken, so that the mixture contained 2·02 equivalents of chromic oxyd for 1 equivalent of magnesia. No magnesium could be detected in the filtrate from the ammonia precipitate. 0·0400 grm. of Cr_2O_3 was held dissolved by the soda, and 0·3393 grm. of Cr_2O_3 was thrown down again with the magnesia when the precipitate, after solution in chlorhydric acid, was treated with soda in excess.

An analysis of the ignited soda precipitate gave the following results :—

		Taken.	Found.		Theory (MgO, $2Cr_2O_3$).
(III.)	MgO	0·0488	0·0492	12·66	11·56
	$2Cr_2O_3$	0·3780	0·3393	87·34	88·44
		0·4268	0·3885	100·00	100·00

IV. 2·0244 grm. of chrome alum and 0·2500 grm. of Epsom salt were taken, so that the mixture contained 2 equivalents of chromic oxyd for 1 equivalent of magnesia. A trace of magnesium was detected in the filtrate from the ammonia precipitate, and 0·0337 grm. of MgO was obtained from the soda precipitate. 0·1167 grm. of Cr_2O_3 was thrown down on boiling the filtrate from the soda precipitate.

V. 2·3967 grm. of chrome alum and 0·3005 grm. of Epsom salt were taken, so that the mixture contained 1·97 equivalent of Cr_2O_3 for 1 equivalent of MgO. No magnesium was detected in the filtrate from the ammonia precipitate. 0·0250 grm. of Cr_2O_3 was thrown down on boiling the filtrate from the soda precipitate.

VI. 1·0120 grm. of chrome alum and 0·5000 grm. of Epsom salt were taken, so that the magnesia should be in large excess, the proportion of magnesia to chromic oxyd in this case being as 2 to 1. 0·0077 grm. of MgO was obtained in the filtrate from the ammonia precipitate, and 0·0765 grm. of MgO in the soda precipitate. No chromium was found in the filtrate from the ammonia precipitate, while 0·1508 grm. was obtained from the soda precipitate. The results may be tabulated as follows:

		Taken.	Found in the soda precipitate.		Theory ($2MgO, Cr_2O_3$).
(VI.)	$2MgO$	0·0809	0·0765	33·66	34·34
	Cr_2O_3	0·1549	0·1508	66·34	65·66
		0·2358	0·2273	100·00	100·00

The soda precipitates, after having been ignited and weighed, were decomposed by fusion with carbonate of sodium and nitrate of potassium in the usual way.* The chromium was then determined as chromate of lead, and the magnesium in the form of pyrophosphate.

It was found to be impossible, in this way, to convert all the chromium into a soluble compound in one operation. On treating the fused mass with water, there remained undissolved, on every occasion, a certain quantity of a fine, whitish-yellow substance, which was so finely divided as to pass at first quite readily through the filter; it could be collected only by returning the filtrate to the filter several times. A second fusion with carbonate of sodium and nitrate of potassium was consequently resorted to in each case, in order to complete the decomposition. The yellow color of the fused mass and of the solutions obtained after these second fusions, showed clearly enough that, whatever the real composition of the unresolved powder may be, a certain proportion of chromium is always

* Fresenius. Quantitative Analysis, p. 372.

contained in it. It should be stated that considerable difficulty was encountered in filtering the chromite of magnesium precipitated by caustic soda, especially in those cases where a comparatively large proportion of chromium remained dissolved in the excess of soda, for hydrate of chromium is gradually deposited from such solutions even in the cold. Toward the close of the filtration a certain portion of the chromium, which was dissolved at first, is in this way precipitated upon the filter and added to the mass of chromite of magnesium.

The foregoing experiments were undertaken at the suggestion of Prof. F. H. Storer, by whom my attention was called to the subject in the autumn of 1867.

Boston, May, 1868.

ART. IV.—*Notices of papers in Physiological Chemistry*—No. II; by GEORGE F. BARKER, M.D.

5. *On the formation of Sugar in the liver.*

(Continued from vol. xlvi, p. 390.)

(25.) On the 23d of March, 1857, BERNARD announced to the Academy* the isolation of the glycogenic matter. His progress was hindered for a long time by the false notion that this substance was an albuminate; but at length he recognized the fact that it was the albuminoid ferment, not the glycogenic substance, which was altered by cooking, and that the latter could be separated from the ferment by solution in hot water. His process for its preparation is as follows: The liver from an animal fed entirely upon meat (though any liver may of course be used) is divided while still warm, into fine shreds, which are thrown into water in active ebullition, to coagulate the ferment. The mass is then bruised in a mortar, mixed with a small quantity of water, and boiled for $\frac{3}{4}$ of an hour. On straining and pressing out, an opaline liquid is obtained, to which is added 4 or 5 times its volume of strong alcohol; an abundant flocculent, slightly yellowish precipitate, is thrown down, which contains sugar, bile, and various nitrogenous bodies, as impurities. It is collected on a filter, washed with strong alcohol, and dried; it then forms a grayish, somewhat gummy mass of crude glycogenic matter. To purify it, it is boiled in a concentrated solution of potassic hydrate for half an hour, diluted, filtered, again precipitated by adding alcohol, collected on a filter, washed with alcohol, re-dissolved in water, neutralized exactly with acetic acid, again thrown down by alcohol, collected and dried. As thus prepared, the glycogenic matter

* C. R., xliv, 578.

resembles very closely hydrated starch; it is neutral, is without odor or taste, produces the sensation of starch on the tongue, and diffuses itself in water, making it strongly opaline. It has no characteristic appearance under the microscope. Iodine produces with it a color, varying from a dark violet-blue to a clear maroon-red. Heated with soda-lime, it gives, when pure, no ammonia, thus proving the absence of nitrogen. It does not reduce alkaline solutions of copper, does not ferment when mixed with yeast, is entirely insoluble in strong alcohol, and is precipitable from its solutions by basic plumbic acetate. It is changed into dextrin and then into sugar, by all those agents which convert ordinary starch into dextrin and sugar; prolonged ebullition with dilute mineral acids, and the action of diastase and analogous ferments, such as exist in the pancreatic juice, the saliva, and the blood, effect this change easily; the opaline character of the solution disappears, it is no longer colored by iodine, it reduces alkaline solutions of copper, and ferments with yeast, yielding alcohol and carbonic acid. The diastasic ferments produce this change in a few minutes at the temperature of the body. Torrefaction, and the partial action of ferments or of dilute acids, change the glycogenic matter into a substance which, like dextrin, is insoluble in strong alcohol, is soluble in water giving a transparent solution, is not colored by iodine, does not reduce copper-tests, does not ferment, and rotates to the right the polarized ray. From the above results, Bernard argues that the formation of sugar in animals should be divided into two parts: the 1st—entirely vital, since it takes place under the influence of life—consists in the production of the glycogenic matter in the living hepatic tissue; the 2d, entirely chemical, is the transformation of this into sugar by means of a ferment. This ferment was at first supposed to belong to the liver, but finding that blood effected the change in a marked degree, Bernard referred it to this fluid. During digestion, the circulation is much more active, and the transformation of the glycogenic substance is correspondingly great; in torpid animals like frogs, on the other hand, where the circulation is slow, the sugar disappears from the liver, while the glycogenic substance remains. If these animals be warmed, however, the sugar re-appears, and so on alternately. The condition of the nervous system also affects the transformation of the glycogenic matter. When the spinal cord is cut or wounded near the neck, below the origin of the phrenic nerves, the activity of the hepatic circulation is diminished, and after 4 or 5 hours there is no trace of sugar in the liver, though glycogenic matter is found there. On wounding the cerebro-spinal axis near the fourth ventricle, the phenomena produced are precisely the

reverse; the abdominal circulation is accelerated, and the glycogenic matter is so largely transformed that it appears in the urine. The paper closes by comparing the liver to a seed; first, a vital action stores up starch in it; then a chemical one changes this starch into sugar.

(26.) On the 1st of June, SANSON announced to the Academy* the discovery of glycogenic matter in the spleen, the lungs, and the kidneys, as well as in venous, arterial, and portal blood.

(27.) At the next session, FIGUIER† communicated some experiments to prove that washing the liver by Bernard's method for forty minutes did not deprive it of sugar. He therefore pulped a liver and washed it until free from sugar; none re-appeared after 24 hours standing. He then washed a liver by Bernard's method for $1\frac{1}{2}$ hours, and on dividing it into two parts, found that one at once reduced ·123 grams cuprous oxyd, while after 24 hours, the other half only produced ·102 grams. In a third experiment a horse's liver, proved to contain sugar, was washed through the portal vein for $2\frac{1}{2}$ hours; on examination, no sugar was found in it, nor did it re-appear after 24 hours. Two other experiments resulted similarly. He expresses the opinion that Bernard's glycogenic matter is only albuminose altered by boiling with potash.

(28.) Some experiments on the conversion of the glycogenic matter into xyloidin, were presented to the Academy June 29,‡ by EUG. PELOUZE. One gram of the pure substance was mixed with concentrated nitric acid; in a few moments it dissolved, and the whole was then poured into water to precipitate the xyloidin. This, after drying, weighed 1·3 grams, the theoretical quantity. It was very combustible, taking fire at 180° C. When the glycogenic matter was treated with dilute nitric acid and heated, it was converted into oxalic acid. On combustion, it yielded carbon 39·8, hydrogen 6·1, oxygen 54·1 = 100, giving the formula $C_6H_{12}O_6$; while that of ordinary starch treated with potassic hydrate and dried at 100° C., has the composition $C_{12}H_{22}O_{11}$. Pelouze had examined other organs of the body, but had failed to find glycogenic matter in any but the liver. On treating the lungs of a calf by Bernard's method, a substance was obtained which at first sight resembled it; this substance, however, could not be changed into sugar and was probably modified albumin. Sanson's results are therefore incorrect.

(29.) In a paper presented at the same session,§ SANSON asserts that ptyalin converts the starch of the food into dextrin, which is absorbed by the abdominal venous system, carried

* C. R., xliv, 1159. † Ib., p. 1213. ‡ Ib, p. 1321 § Ib., p. 1323.

into the tissues by the general circulation, and converted into glucose. Animals nourished on meat, therefore, get dextrin from their food, a fact which explains the contradictory statements as to the existence of fermentable matter in the blood. Sanson shows that the action of the blood-ferment on dextrin produces sugar, by taking 500 grams arterial blood from the carotid of a horse, defibrinating it at once by whipping, and allowing it to stand 24 hours; at the end of this time it contains sugar and readily ferments. He maintains that the liver forms neither glycogenic matter nor sugar, but simply uses that carried to it in the blood; more sugar is found there than elsewhere, because the circulation is slower.

(30.) Sanson was immediately followed by BERNARD,* who asserted that this animal starch existed only in the liver. His experiments show that when rabbits are fed on carrots a large quantity of glycogenic matter is found in the liver, but none in any other organ. When the animal is fed on grain, the case is more complicated, as soluble starch colorable by iodine is then carried into the body. In a former memoir, it was stated that starchy matters did not pass into the blood as dextrin; but the process used was imperfect, and later researches have proved the presence of dextrin in both blood and muscles. Sanson has simply confirmed this fact. Two points were then noticed: I. The formation of glycogenic matter in the liver is constant, and is entirely independent of the external supply of dextrin or sugar. The blood of a rabbit fed for 3 or 4 days on wheat or oats contains dextrin; but this is not true with an animal fed on carrots. Moreover on changing the food of the first rabbit, the dextrin disappears. The glycogenic function, on the other hand, is never altered; it persists always, no matter how the food varies. The production of dextrin in the blood by certain aliments, is of course interesting; but only so as an independent fact. The presence of dextrin in the organs of horses fed on oats or other grain, has been confirmed at the Ecole Imperiale at Alfort, in connection with M. Bouley; but no conclusion as to the formation of sugar can be drawn from these accidental results. Nor has dextrin been detected in beef or mutton from the butcher's; which should not be the case were it present. II. A second proof of this function is the fact that it diminishes or disappears under the influence of many morbid conditions. Hence, the livers of hospital cadavers contain no sugar, and the livers of executed criminals must be used for its detection. The same fact is true of animal starch; whenever the animal is sick, none is formed, even when food is taken as usual. Experiments at the veterinary school

* C. R., xliv, 1325.

above mentioned, upon horses who though diseased yet ate well, showed no glycogenic matter in the liver, though sugar formed from the food was found there. A comparative experiment, witnessed by Prof. Schmidt of Dorpat, was made on two vigorous horses; one was healthy, but was to be killed because of an old paralysis of one fore leg, of traumatic origin; the other had had an acute inflammation of the foot-joint for 3 years, also from an injury. Both were fed on hay and oats and ate well, though the second animal had some fever. Three or four hours after eating they were killed. On dissection, digestion appeared to have taken place perfectly; the lacteals were full of chyle, and the urine was alkaline. The livers were removed and at once examined; that of the well horse furnished a strongly opaline decoction, very rich in glycogenic matter, while that of the arthritic animal gave a limpid solution containing not a trace. The flesh of both animals contained dextrin and sugar, from the food. It appears, therefore, that on the one hand, the formation of animal starch in the carnivora takes place upon food entirely free from carbohydrates, and on the other, that it ceases in the diseased herbivora, even with food rich in these substances. Beside the febrile state, all exhausting conditions, such as prolonged suffering, arrest this production; hence, healthy animals must be used for its demonstration. The paper closes with a new method for the preparation of the glycogenic matter. A filtered decoction of the healthy liver is mixed with glacial acetic acid in excess; a whitish precipitate of almost pure animal starch falls, while the albuminoid matters being soluble, remain dissolved. A diseased liver gives no precipitate; nor does the solution from any other organ. If a liver solution be divided into two parts, and acetic acid added to one, a heavy precipitate falls; the other, on the addition of a little saliva, becomes transparent and gives no precipitate. In this way, therefore, the presence of the glycogenic matter may be easily and rapidly recognized.

(31.) On the 27th of July, FIGUIER* asserted that the sugar which is contained in portal blood is one of the non-fermentable sugars. By a new process, he had isolated a sugar from the jugular and femoral veins of a dog, identical with that in portal blood; it reduced the copper-tests, was not precipitated by basic plumbic acetate, and was not fermentable until boiled with $\frac{1}{100}$ of sulphuric acid. When this sugar remains for a time in the system, it becomes fermentable; hence, kiestein-urine sugar, chyle sugar, and the sugar of eggs, are fermentable. Bernard's sugar, therefore, has its origin in the intestinal canal; and Figuier detected there a sweet substance,

* C. R., xlv, 132.

not precipitable by basic plumbic acetate and not fermentable. He enumerates three varieties of sugar: 1st, that of the intestines, not precipitable or fermentable, does not reduce copper-tests; 2d, that of the portal blood, which reduces copper-tests, and is not directly fermentable, but becomes so on boiling with acid; and 3d, that of the liver and chyle, which reduces copper and ferments directly. He asserts that by the action of a boiling solution of potash upon albumin, a sugar capable of reducing the copper-test is formed. Since as in animal fluids like milk, sugar and the albuminates occur together, they may be mutually dependent.

(32.) A paper by BONNET, confirming Bernard's results, was read at the same session.* Its conclusions are; 1st, there is no sugar in the portal blood of animals fed on meat, though the liver and hepatic blood contain it; 2d, sugar is formed in the liver posthumously; 3d, there is no sugar in the general circulation of animals on a meat diet; 4th, on starchy food after digestion is completed there is no sugar in portal blood; and 5th, if, as Figuier says, the liver actually converts the portal substance into sugar, this is a true function.

(33.) In a third paper read at this session,† SANSON maintains that glycogenic matter, being intermediate between starch and glucose, must vary in its properties according to the stage of conversion; and that the matter which he obtained from the tissues was true glycogenic matter, as, under the influence of diastase, it was converted into sugar, from the fermentation of which he had obtained several specimens of alcohol. Bernard has confirmed what Pelouze had denied, the existence of dextrin in the blood. If dextrin and salivary diastase both exist in the blood, what more is needed for the production of glucose?

(34.) In another paper, read Sept. 7th,‡ he says (1) Pelouze's conversion of the glycogenic matter into xyloidin does not prove it to be starch, since dextrin may be thus converted; (2) his analysis fails of proving it, the formula given being that of glucose; moreover, starch and dextrin being isomeric, analysis cannot decide between them; (3) acetic acid precipitates dextrin as well as starch; (4) the glycogenic matter of the liver is chemically dextrin, like that in the blood of other organs; (5) dextrin is found in the portal blood and in that of the general circulation of animals fed exclusively on meat; (6) the dextrin in meat is spontaneously converted into sugar, when the meat is cut fine, and exposed to the air for 48 hours; (7) no necessity exists for the assumption of a glycogenic function; (8) Bernard's experiments with carrots are unfortunate,

* C. R., xlv, 139. † Ib., p. 140. ‡ Ib., p. 343.

since these vegetables contain starch, from which came the dextrin which he found in the liver.

(35.) The following results of experiments by Coze were read also at the session of Sept. 7th:* (A) Under physiological conditions: 1st, the kind of death affects the quantity of sugar in the liver; the slower the death, the smaller being the quantity; 2d, the proportion of sugar in arterial blood is to that in the liver as 1 : 11. (B) Under pathological conditions: 1st, under the action of morphine hydrochlorate; (*a*) the sugar in the liver more than doubles, rising from 0·59 to 1·39; (*b*) the same is true of arterial blood, the amount increasing from 0·05 to 0·11; (*c*) the proportion of liver sugar to arterial sugar remains the same, 1 : 12, and hence pulmonary oxydation is not affected; (*d*) the increase of sugar under the influence of morphine is an argument against the employment of opium in treating diabetes, and explains the want of success met with in its use; (*e*) no sugar is found in the urine. 2d, under the action of tartar-emetic; (*a*) the liver sugar remains unaltered; (*b*) it is doubled in arterial blood, being 0·10 instead of 0·05; (*c*) the proportion between them is 1 : 6 instead of 1 : 11; (*d*) the combustion of sugar in the lungs is hindered,—thus accounting for the efficacy of this substance in pneumonia;—(*e*) no sugar appears in the urine. The action of tartar-emetic upon the production and the destruction of glucose is precisely the reverse of the action of morphine.

(36.) In a paper communicated to the Academy Oct. 19th,† Bonnet contends that the liver has a true glycogenic function, nothing analogous existing elsewhere in the animal economy; that the posthumous formation of sugar is established, the facts quoted to disprove it being inconclusive physiologically, since a liver-hash is not the normal liver; that the two sugars of Figuier, one in the intestinal canal, the other in the portal vein, have no existence; that even if a non-fermentable sugar be admitted to exist in the portal blood, the fact remains that the liver in converting this into a fermentable sugar, exercises a true glycogenic function; that the economy contains fatty, albuminoid and other matters, the elements of which by being differently grouped molecule by molecule, under certain unknown influences, may yield glycogenic matter; but that this result, performed outside the body by chemical action, does not represent a physiological fact; that any substance serving as a basis of Sanson's opinions may be in vain sought for in portal blood; that dextrin is transformed by fuming nitric acid directly into oxalic acid, without forming xyloidin; that the formula of ordinary starch is not that of dextrin, anhydrous

* C. R., xlv, 345. † Ib., p. 573.

starch and dextrin being $C_{12}H_{18}O_9$, while glycogenic matter is $C_6H_{12}O_6$; that the latter resembles hydrated starch rather than dextrin, since like starch, it forms a paste with water and is colored blue by iodine, while dextrin does not form a paste and is not colored by iodine; that glycogenic matter when isolated, does not resemble dextrin; that Sanson is the only chemist who has found dextrin in the blood of the carnivora; that he (Bonnet) has not found it himself, and does not believe in its existence; that dextrin, which is so easily transformed by ptyalin, cannot be taken as food and enter the system without change; that chopped meat does not contain sugar after exposure to the air; that when food rich in sugar is taken, or when sugar is formed from starchy food by the diastase of the intestinal juices, it must be re-converted to dextrin, according to Sanson; that no substance capable of producing this change is known; and that even admitting that dextrin exists in portal blood, its conversion into sugar by the liver is a true glycogenic function.

(37.) Moos published a paper in 1858,* in which he shows that when the pneumogastric nerves of healthy dogs who excrete 0·7 grams of liver sugar for each half kilogram of bodily weight are cut, this amount of sugar constantly diminishes, reaching even 0·1 gram, but never entirely disappears. In rabbits, too, when this nerve is severed, the production of sugar is very slight. This result he ascribes partly to the important diminution in bodily weight and in all the secretions, including the formation of sugar, which takes place on dividing these nerves; and partly to the diminished or altogether suspended nutrition. Electrical irritation of the spinal cord of frogs produces increased urination with diabetes, in a very short time. On ligating the vessels of the liver, though the amount of urine continues large, the sugar disappears. When the spinal cord of these animals is divided, the sugar formation is entirely arrested in the course of fifteen to twenty hours.

(38.) In a paper published at the same time,† KEKULÉ recommends Bernard's method as the most convenient one for the preparation of glycogen from the liver. A half-hour's boiling with strong potassic hydrate is sufficient to deprive it entirely of nitrogen. The ash, which consists of lime-salts, may be entirely removed by repeated solution either in strong acetic or in cold dilute nitric acid, and precipitation with alcohol. Kekulé confirms the views of Bernard, Hensen, and Pelouze relative to the properties of glycogen. It is white, and completely amorphous, and is colored violet or reddish-

* Verh. Med. Heidelberg, Jan., 1858; Jahresb., 1858, 568.
† Ib.; Jahresb., p. 570.

brown by iodine. Its aqueous solution is opalescent, but becomes rapidly clear on boiling with dilute sulphuric acid; though prolonged boiling is necessary to convert it into sugar. His analysis yielded the formula $C_6H_{10}O_5$. The liver of dogs contained on an average two per cent of glycogen.

(39.) In a communication by POISSEUILLE and J. LEFORT to the Academy,† they state that in some physiological researches made by one of them, certain results were obtained, which were inexplicable except upon the assumption that glucose is destroyed in the lungs. But this is admitting the glycogenic hypothesis; and since the radical objections made to it are enough to shake the strongest faith in it, they have undertaken, before giving their assent to it, to re-examine the whole subject *ab ovo*, seeking for glucose not only in the liver, but also in other organs of vertebrate animals. The livers of both fresh and salt water fish afforded, in some instances, from 0·484 to 1·5 per cent of sugar; but the intestines, spleens, roes, ovaries and muscles yielded none. In other cases, no sugar was detected anywhere. The liver of frogs gave 0·315 to 0·632 of sugar; the viscera and muscles none. Birds gave the same result as fishes; the liver contained from 0 to 2·164 per cent of sugar, the viscera none. With mammals, the liver of hares gave none, of roe-deer 1·092, of three rabbits 1 to 1·163, of three cats ·807 to 2·305, of two dormice in the hybernant state 0·624 per cent of sugar; the other viscera and the muscular tissue gave none. Since sugar is destroyed when left in contact with decomposing animal matter, the authors infer that when a liver affords no sugar, it is because of such a change. They state that they have found sugar in the juice of the horse-flesh upon which their dogs were fed, as also in mutton, veal, beef and pork, though in minute quantity, a few milligrams for 100 grams of meat. The following experiments were made to ascertain the origin of this sugar: (A.) Dog, weight 33 kilos. fasting for 60 hours; had been fed for a month with 3 to 4 kilos. horse-meat daily. The liver yielded 1·487 per cent glucose, the lymph of the thoracic duct 0·141, hepatic blood 0·821, portal, carotid, inferior vena caval blood, small intestines, mesenteric ganglions, none; blood of right ventricle, traces; of left ventricle, lungs, spleen, kidneys, brain, urine, muscle, none. None of these fluids contained dextrin; the same is true of the organs, except the liver,—the decoction of which was slightly reddened by iodine,—and the muscles, in which the coloration was more decided. Without much doubt the portal blood, though it contains no sugar, yet contains a substance which is transformed into sugar; but it

† C. R., xlvi, 565, March 22.

is in the liver that this change is effected. The sugar found in the lymph must come from the intestines or the liver; but since the former contain none, it evidently must be taken up by the lymphatics from the liver-tissue. (D.) Horse in full digestion; had eaten 10 liters of oats a day for the two previous days, and on the day of the experiment the same quantity at two different hours. The liver furnished 2·292, hepatic blood 1·128, chyle 0·222, lymph (from the vessels of the head and neck) 0·442, carotid blood 0·069, jugular blood above the ligature 0·050, portal blood above the ligature, 0·065, inferior vena cava below the ligature 0·057 per cent of sugar; mucus of the small intestine, the intestine itself, muscular tissue of the heart, locomotor muscles, traces; pancreas, doubtful; mesenteric ganglions, spleen, kidneys, brain, lungs and urine, none; synovia from the knee joint 0·142 per cent. As before, sugar exists in the liver, in hepatic blood, and in the contents of the thoracic duct, but now also in arterial and portal blood, the intestinal walls, etc. Moreover, since the quantity of sugar found in the lymph in these experiments is 0·166 per cent for the dog, and 0·442 for the horse, while the quantity found in a lymphatic of the mesentery of a cow by the author of intestinal glycogeny is only 0·186 per cent, the conclusion is easily reached that it is the lymph and not the chyle which furnishes the sugar, and hence, that this is not produced in the intestine. To sum up, the liver of fishes, reptiles, birds and mammals, examined immediately after death, always contains sugar, while its presence in other organs is accidental and temporary, due to special physiological conditions; consequently, "le foie seul forme du sucre."

(40.) In a subsequent paper,* these authors give some results,—obtained at Alfort,—of the examination of the intestinal chyle and the lymph of other parts of the body, of a bull killed while in full digestion. The chyle was collected from a large mesenteric vessel coming from the intestine; and 25 grams of lymph were obtained from a lymphatic vessel near the primitive carotid artery; from this artery itself 300 grams of blood were taken. These liquids were prepared the same day and examined on the next. In the arterial blood 0·073 per cent of sugar was found; in the chyle from the intestinal vessel, 0·123; in lymph taken from the neck, 0·266. The lymph, therefore, contains more sugar than the chyle; the reverse of this should be true, however, were glucose formed in the intestinal canal. It is clear, too, that during digestion all the sugar yielded by the liver is not destroyed in the lungs; but that a portion passes into the arterial blood and thus into

* C. R., xlvi, 677, Apr. 5, 1858.

all the organs of the body; whence it is at length absorbed by the lymphatics, and poured, partly into the right subclavian vein directly, partly into the left subclavian by means of the thoracic duct. The lymphatics thus supply sugar to the superior vena cava, as the hepatic veins do to the inferior. In the carnivora, this action is temporary, taking place only during digestion; in the herbivora, since they are always in digestion, it is permanent.

(41.) In a report to the Paris Academy of Medicine upon the formation of glycogen in the animal organism,* POGGIALE states as the result of his experiments, that the preparation of glycogen—which in its properties stands between starch and dextrin—by means of glacial acetic acid, is to be preferred to any other method. As contained in a concentrated decoction of the liver, or in muscular juice, this substance, after gently heating with saliva, passes readily into fermentation, with the evolution of carbonic acid. With animals fed exclusively on meat, glycogen is found only in the liver. It exists abundantly in the liver of the herbivora, but occurs in other organs only when the food is rich in amylaceous matters.

(42.) Dr. F. W. PAVY, in a communication to the Royal Society, June 17, 1858,† claims that the question under discussion is not whether sugar is found in the animal organism independently of saccharine food; for this he is ready to grant. But it is whether the sugar thus found *after death*, is really present during life, or is the result of a *post-mortem* change. As early as Feb., 1854, he had noticed that scarcely a trace of sugar existed in blood removed by catheterizing the right ventricle during life. Sometime afterward, on repeating this experiment, his attention was fixed upon this point, and an investigation was undertaken to elucidate it. As the result of more than 60 observations, he now asserts that the condition of the blood after death cannot be taken as an indication of its state in life. He finds that in blood drawn from the right ventricle of an animal in a tranquil state, hardly a trace of sugar can be detected; while in blood collected from a fine incision in this organ immediately after death, sugar is present. In one experiment barely a trace existed in the blood during life, but nearly 1 per cent was found in that collected after death. It was natural to pass next to the organ in which, as is alleged, sugar is produced. As Bernard has shown that a substance readily convertible into sugar exists in the liver, it was necessary first to search for some substance which, when introduced into this organ, would check this post-mortem transformation. Potassic hydrate was found to answer this

* J. pharm., III, xxxiv, 99; Jahresb., 1858, 569. † Proc. Roy. Soc., ix, 300.

purpose and to exert no injurious effect; instantly after death therefore, a strong potash solution was injected into the liver by the portal vein. In this liver no sugar was detected; though another liver not thus treated, gave in a short time the ordinary sugar-reaction. If one portion of a liver be thus injected, it shows no sugar, while a second portion not thus treated is found to be saccharine. Abstraction of heat from the liver, immediately after death, yields the same result. In one case, a dog was killed, a piece of the liver instantly sliced off and thrown into a freezing mixture of ice and salt; the absence of sugar in this portion was almost complete; while the rest of the organs, which remained a short time in the animal, afforded 2·96 per cent of sugar. Division of the spinal cord, as already noticed by Bernard, suspends the production of sugar. This fact Pavy explains by showing that the body is thus cooled down so low—to 70° F.—that the post-mortem change does not go on with sufficient rapidity to afford sugar enough for detection. After a longer time, however, the liver becomes strongly saccharine. This view is sustained by the fact that if the temperature of the animal after section of the spinal cord, be artificially maintained, the ordinary amount of sugar is found in the liver; and also by the farther fact that with rabbits whose coats are oiled, so that on exposure to cold their temperature falls, the same result takes place and sugar is no longer found. With frogs, the amount of sugar in the liver is dependent upon their temperature when killed; a fact noticed by both Pavy and Bernard about the same time. As already stated, Bernard explains this fact by the diminished activity of the abdominal circulation; Pavy considers that no sugar appears because the post-mortem change is prevented by this diminution of temperature. Hence, Bernard's name, "glycogenic matter," implies a conversion not physiologically true; Pavy proposes therefore to call this substance "Hepatine." He shows that the amount of hepatine is greater on a vegetable than an animal diet, that it increases when sugar is mixed with the food, and that it causes the liver to vary in relative weight. The liver of 11 dogs fed on meat weighed $\frac{1}{30}$ that of the animal, the percentage of hepatine being 6·97; that of five dogs on vegetable food, weighed $\frac{1}{11}$ that of the animal, and contained 17·23 per cent of hepatine; while that of four dogs fed on meat and given $\frac{1}{4}$ lb. cane sugar daily, was $\frac{2}{33}$ that of the animal, the hepatine being 14·5 per cent. What the destination of hepatine is, and by what means it resists transformation during life, are questions which the author leaves for future researches to determine. Possibly some analogy to the latter condition is found in the fact that saliva, which readily converts neutral hepatine into sugar, is

entirely without action if it be either acid or alkaline. The trace of sugar (0·047 to 0·073 per cent) found in right ventricular blood, is caused, as Pavy supposes, by the escape of a small amount of hepatine from the liver; since any disturbance in the circulation, either by congestion or otherwise, causes this amount of sugar to increase. Moreover, on artificially introducing hepatine into the blood, this fluid becomes saccharine; and if sufficient be injected, strongly marked diabetic urine is voided. If an animal be killed, and the circulation be maintained by artificial respiration, the urine becomes strongly saccharine by the sugar produced by the post-mortem transformation of hepatine.

(43.) On the 11th of July, 1859, Bernard presented to the Academy* a letter from C. Schmidt of Dorpat communicating the results of his examination of the portal and hepatic blood of three dogs, two of which were digesting meat, the other had fasted for two hours. The portal blood contained no sugar in either case; the hepatic nearly one per cent of the dry residue, for the first two dogs, and about $\frac{1}{2}$ per cent for the third. Or more exactly, 0·93 and 0·99 for the dogs in digestion, 0·51 per cent for the fasting animal; thus confirming Lehmann's results.

(44.) In a paper read on the 1st of August,† Berthelot and de Luca give the results of a research to determine the nature of the glucose yielded by the transformation of Bernard's glycogenic matter. It has never been shown whether this sugar is identical with any of the known varieties, such as grape-sugar, malt-sugar, levulose, lactic glucose, etc., or whether it is a new one. Their glucose was obtained by the action of chlorhydric acid upon the glycogenic matter from the liver of a rabbit. They obtained in a well crystallized form, the compound of this glucose and sodic chlorid, and subjected it to a systematic examination. The compound forms bulky crystals, limpid and colorless, capable of reducing the copper-tests, and of fermenting with yeast. They are rhombohedrons of 78°, and their solution rotates to the right +47°, this rotatory power being considerably greater a few minutes after the crystals are dissolved, than subsequently. On analysis they gave 8·3 per cent chlorine, which corresponds to the formula $(C_6H_{12}O_6)_2 H_2O+NaCl$. They agree in all their properties with those of the compound of grape-sugar and sodic chlorid, as first described by Peligot and Pasteur. It is therefore certain that hepatic glucose is identical with the ordinary glucose of grapes and of diabetes.

* C. R., xlix, 63. † Ib., p. 213.

(To be continued.)

Art. V.—*Derivative Hypothesis of Life and Species;* by Professor Owen, F.R.S.*

§ 422. *Biological Questions of* 1830.—At the close of my studies at the Jardin des Plantes, Paris, in 1831, I returned strongly moved to lines of research bearing upon the then prevailing phases of thought on some general biological questions.

The great Master in whose dissecting-rooms, as well as in the public galleries of Comparative Anatomy, I was privileged to work, held that 'species were not permanent:' and taught this great and fruitful truth, not doubtfully or hypothetically, but as a fact established inductively on a wide and well-laid basis of observation, by which, indeed, among other acquisitions to science, Comparative Osteology had been created. Camper† and Hunter‡ suspected that species might be transitory; but Cuvier, in defining the characters of his *Anoplotherium* and *Palæotherium*, &c., proved the fact.

In this truly scientific labor the law of the subordination of the different organic characters to the condition of the whole animal was first appreciated, clearly enunciated, and its application shown to the reconstruction of lost species from fragmentary remains. The importance of this generalization may be paralleled with that of the principle of equivalents in chemical science.

Of the relation of past to present species, and the conditions of their succession, Cuvier had not an adequate basis for a decided opinion. Observation of changes in the relative position of land and sea suggested to him one condition of the advent of new species on an island or continent where old species had died out. This view he illustrates by a hypothetical case of such succession,§ but expressly states:—'Je ne prétends pas qu'il ait fallu une création nouvelle pour produire les espèces aujourd'hui existantes, je dis seulement qu'elles n'existoient pas dans les mêmes lieux, et qu'elles ont dû y venir d'ailleurs.'‖

Geoffroy Saint-Hilaire, whose discussions with his colleague in the 'Académie des Sciences' made its annals of 1830 memo-

* As there has been much discussion with regard to the relation of Prof. Owen's views on the origin of species to those of Dr. Darwin, we reprint this chapter (the 40th) from the forthcoming edition of his Anatomy of Vertebrates, from a pamphlet sent us by the author, omitting four of the longer notes.—Eds.

† CCXCII″. ‡ CCXCIII″, and other authors cited in CXXXIX, p. xlv.

§ CXXXIX, tom. i, p. lxiii. ‖ Ib.

(Nos. denoting 'Works' in the 'Lists of Authors cited' in 'Anatomy of Vertebrates,' Vols. I, II, and III.)

rable, equally rejecting the idea of new creations,* opposed to Cuvier's inductive treatment of the question the following expression of belief:—Je ne doute pas que les animaux vivants aujourd'hui ne proviennent, par une suite de générations, et sans interruption, des animaux perdus du monde antédiluvien.'† But with regard to the demonstration of the proposition, of the truth of which he could not entertain a doubt, Geoffroy Saint-Hilaire expressly states:—'Je crois que les temps d'un savoir véritablement satisfaisant en géologie ne sont pas encore venus.'

The main collateral questions argued in these debates, to some of which I listened, and to all the reports and consequent pamphlets relating thereto devoted intense attention, appeared to me to be the following:—

Unity of Plan or Final purpose, as a governing condition of organic development?

Series of Species, uninterrupted or broken by intervals?

Extinction, cataclysmal or regulated?

Development, by epigenesis or evolution?

Primary life, by miracle or secondary law?

On returning home and resuming office with additional duties at the Royal College of Surgeons, I was guided in all my work with the hope or endeavor to gain inductive ground for conclusions on these great questions.

§ 423. *Homology or Teleology?*—Cuvier held the work of organization to be guided and governed by final purpose, or adaptation, expounding this principle under the terms 'conditions of existence' and 'correlations of structure.' Geoffroy denied the evidence of design, and protested against the deduction of a purpose as, e. g., from the coëxistence of a valve with a definite course of fluid: he contended for the principle which he called 'unité de composition,' as the law of organization. Most of his illustrations were open to the demonstration of inaccuracy, and his arguments to the refutation which they received from Cuvier in the debates in question: the logic, and, as it seemed, the facts, were on the side of teleology. The figurative language, moreover, in which contemporary anatomists had expressed their views of a principle akin to Geoffroy's was ill-calculated to enlist supporters. The expressions by which disciples of the school of Schelling illustrated, in the animal structures, the transcendental idea of 'the repetition of

* 'Or, cette proposition, déja contraire aux plus anciennes données historiques, répugne tout autant aux lumières de la raison naturelle qu'aux spéculations plus réfléchies des sciences physiques.'—CCLXXXVII", p. 210.

† Also, more decisively:—'Les animaux perdus sont, par voie non interrompue de générations et de modifications successives, les ancêtres des animaux du monde actuel.'—CCLXXXVII", p. 208.

the whole in every part,' operated disadvantageously to the calm inquiry into the prime question at issue. To Cuvier this language seemed little better than mystical jargon, and he alluded to it with transparent contempt.* When he did extend inferences from comparative anatomy beyond the adaptation of structure to function, Cuvier went not beyond a recognition of what I have since termed 'special homologies:'† and this lowest degree of correspondence he explained on the ground of the subserviency of such homologous parts to similar ends in different animals;‡ viewing them, in fact, in that relation which I express and contrast by the term 'analogies.'§ With Cuvier answerable parts occurred in the zoological scale because they had to perform similar functions.

Most of my fellow-students at the Garden of Plants, in 1830, and some subsequent fellow-laborers, Johannes Müller, Rud. Wagner, Milne-Edwards, Agassiz, implicitly accepted this explanation of the fact of answerable bones and other parts occurring in different species.

After the publication of the 'Memoir on the Pearly Nautilus,' and of those on Monotrematous and Marsupial generation, which subjects Cuvier had strongly recommended to my attention, the question of the condition or law of special homologies pressed itself upon me, more especially in connection with the task of arranging and cataloguing the osteological part of the Hunterian Museum.‖ As my observations and comparisons accumulated, with *pari passu* tests of observed phenomena of osteogeny, they enforced a reconsideration of Cuvier's conclusions to which I had previously yielded assent. To demonstrate the evidence of the community of organization, I found that the artifice of an archetype vertebrate animal was as essential as that of the archetype plant had been to Goethe in expressing analogous ideas; and as the like reference to an 'ideal type' must be to all who undertake to make intelligible the 'unity in variety' pervading any group of organisms.¶ From the demon-

* 'Quant à M. Oken, il déclare les pièces en question les parties écailleuses des temporaux, ou, selon son langage mystique, "la fourchette du membre supérieur de la tête."—Cet humérous de la tête de M. Oken devient pour M. Spix le pubis de cette même tête; ou, pour parler un langage intelligible, un des osselets de l'ouie, savoir le marteau.'—CXXXIX, tom. v, 2e partie, p. 85.

† CXL, p. 7.

‡ 'Ce n'est qu'un principe subordonné à un autre bien plus élevé et bien plus fécond, à celui des conditions d'existence, de la convenance des parties, de leur coördination pour le rôle que l'animal doit jouer dans la nature. Voilà le vrai principe philosophique d'où découlent la possibilité de certaines ressemblances.'—CCXCIV″, p. 9.

§ CXL, p. 7. ‖ XLIV.

¶ Such 'ideal type' must not be confounded with the so-called 'types' suppose to be exemplified by certain living species. Arguments against the latter vague and ill-defined ideas are of no weight against the former, and indicate a certain obtuseness of apprehension in the objector. See CCCXXVI″, p. 31.

stration of this principle, which I then satisfied myself was associated with and dominated by that of 'adaptation to purpose,' the step was plain—to me inevitable—to the conception of the operation of a secondary cause of the entire series of species, whether of plants, or vertebrates, or other groups of organisms, such cause being the servant of predetermining intelligent Will.*

But, besides 'derivation' or 'filiation,' another principle influencing organization became recognizable in the course of studies and researches on Invertebrate animals. To this principle, as more especially antagonistic to the theological idea, I gave the name of 'irrelative repetition;' sometimes also, as it prevailed most in plants and zoophytes, of 'vegetative repetition.'† The demonstrated constitution of the vertebrate endoskeleton, as a series of essentially similar segments, out of which, as corollary, came the power of enunciating not only 'special' but 'general' and 'serial' homologies, appeared to me to illustrate also the law of irrelative repetition. The recurrence of similar segments in the spinal column and of similar elements in a vertebral segment, struck me as analogous to the repetition of similar crystals as the result of polarizing force in the growth of an inorganic body.‡

Accordingly, these results of extensive, patient, and unbiassed inductive research—or, if there were a bias, it was toward Cuvier—swayed with me in rejecting the principle of direct or miraculous creation, and in recognizing a 'natural law or secondary cause' as operative in the production of species 'in orderly succession and progression.'§

§ 424. *Succession of Species, broken or linked?*—To the hypothesis that existing are modifications of extinct species Cuvier replied, that, in every mooted form of transmutation, the species were made to alter by small degrees, and that, therefore, traces of such gradual modifications were due from the fossil world:—'You ought,' he said, 'to be able to show, e. g., the intermediate forms between the Palæotherium and existing hoofed quadrupeds.'‖

The progress of Paleontology since 1830 has brought to light many missing links unknown to the founder of the science. My own share in the labor led me, after a few years' research,

* CXLI, (1849) p. 86.
† CCXLIX, p. 641 (1843;) and vol. i, Preface, p. ix.
‡ CXL, p. 171. § CXLI, loc. cit
‖ 'Cependant on peut leur répondre, dans leur propre système, que si les espèces ont changé par degrés, on devroit trouver des traces de ces modifications graduelles; qu'entre la palæotherium et les espèces d'aujourd'hui l'on devroit découvrir quelques formes intermédiaires, et que jusqu'à présent cela n'est point arrivé.'—CXXXIX, tom. i, p. lvii.

to discern what I believed, and still hold, to be a tendency to a more generalized or less specialized, organization as species recede in date of existence from the present time.* Even instances which to some have appeared to oppose the rule, really exemplify it. The little marsupial carnivore, e. g., of the Purbeck beds, *Plagiaulax* (p. 294, fig. 234,) retained the typical number of premolars (p. 1–4,) all of them being carnassials: the more modified pliocene *Thylacoleo* had them reduced to the last (p. 4, fig. 233.) So likewise in the latter placental *Carnivora*, the eocene form *Hyænodon*, fig. 266, had the typical number of teeth, the three true molars here showing the carnassial form: in the existing Hyæna and Felines the carnassials are reduced to, or concentrated in, a single molar. The oolitic *Phascolotherium*, with the typical marsupial number of teeth, shows less differentiation in their form than in modern Opossums and Dasyures: the oolitic *Amphitheria* and *Spalacotheria* manifest an earlier and more generalized type of dentition in the great number and similarity of character of their small molars. Both *Anoplotherium* and *Palæotherium*, with the majority of eocene placental Mammals, had the type-dentition of diphyodonts.†

The two notable examples of Cuvier's powers of restoration, viewed as Pachyderms, must have seemed widely different from any of the existing species of the order, and were so deemed. The *Anoplotherium* more especially, among its singular peculiarities, unexpectedly exemplified one dental character, previously known only in the human subject. These seeming anomalies, however, lost much of their import as evidence of insulated form, or special creation, when they came to be viewed by the light of the law of the 'more generalized character of extinct species.' Such law in its application to *Anoplotherium* also exemplifies the analogy between the earlier species of a class and the earlier stages of a fœtus. When, for example, the divided metapodials, the persistent upper incisors, and the hornless cranium of the Anoplothere were recognized as retentions of 'fœtal peculiarities' of existing ruminants,‡ that extinct species was seen to favor rather than oppose the idea of organization by secondary law.

* CCXLIX, Ed. 1843, pp. 129, 165; Ed. 1855, pp. 223, 332, 342. CLXXX, and XVII′, pp. 1, 361, *passim*. Agassiz had been struck by indications of the same law in fossil fishes, and expressed it by the analogy of fœtal and mature structures (CCCXXIX″, (1844) p. xxvi), and this, in some degree, is true. The earlier forms of *Mammalia*, however, are not toothless, have rather an excess of teeth as compared with later and modern forms; but they exemplify, in the main, a more 'generalized' type.

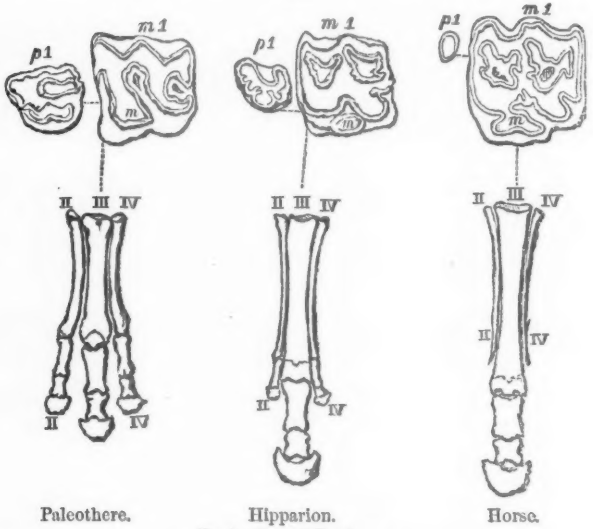
† v. p. 524, CLXXX, p. 361.

‡ CLXXX. p. 367.

The discovery of the remains of the *Hipparion** supplied one of the links required by Cuvier, between the *Palæotherium* and the Horse of the present day, and it is still more significant of the fact of filiation of species that the remains of such three-toed horses are found only in deposits of that tertiary period which intervene between the older palæotherian one and the newer strata in which the modern Horse first appears to have lost its lateral hooflets. These relations I illustrated in my Lectures on Fossil Mammalia at the School of Mines (1857) by the diagram, fig. 614.

Other evidences of gradation, in the case in question, have been brought to light. The molar series of the Horse includes six large complex grinders, individually recognizable by developmental characters as they are symbolized in fig. 280, p. 352. The representative of the first premolar is minute and soon shed. Its homologue in *Palæotherium* is functionally developed and retained, the type-dentition being adhered to.† In *Hipparion*, d_1 is succeeded by a p_1‡ smaller than in *Palæotherium*, but functional, with inflected folds of enamel on the grinding surface,

614.



Paleothere. Hipparion. Horse.

Derivation of Equines.

* CCCIII", tom. ii. p. 25 (1832). Another species was discovered in the Miocene at Eppelsheim—the '*Hippotherium*,' of Kaup; a third in deposits of similar age on the Sewalik Hills; a fourth, *Hipparion prostylum* Gv., at Vaucluse, in the southeast of France, in deposits 'peut-être plus récents que la molasse dans ces localités.'—CCCXXX", p 432.

† v. Pl. 35, figs. 4, 5, 6.

‡ CCCII", Pl. 19, figs. 1, 1*a*.

and permanent. It exemplifies a condition intermediate to that in *Palæotherium* and *Equus*. It is not that the jaws of the Horse are too short to hold the full complement of grinders; on the contrary they are relatively longer than in the Palæothere, being specially produced between the grinders and cutters: the first grinder might seem, indeed, to have been taken away in order to add to the space for the application of the 'bit.' The transitory and singularly small and simple denticle, fig. 614, p_1, compared with the large contiguous massive molar, m_1, in the Horse, exemplifies the rudiment of an ancestral structure, in the same degree as do the hoofless 'splint-bones,' ib., *Equus*, II, IV: just as the spurious hoofs dangling therefrom in *Hipparion*, ib. II, IV, are retained rudiments of the functionally developed lateral hoofs in the broader foot of *Palæotherium*, ib. II, IV.

Other missing links of this series of species have been supplied; as, e. g., by the *Paloplotherium** of the newer eocene of Hordwell, Hants., by the *Palæotherium aurelinense* from the 'molasse marine' of Orleans,† and by the *Palæotherium hippoïdes* of the lacustrine calcareous beds of Sansan, all which deposits are miocene, or are transitional between eocene and miocene. In the first-cited example, the swollen termination of the lobe of the molar, answering to *c*, *m*, fig. 268, remains longer as a detached column, *m*, fig. 269. In the two other Palæotherioids, the whole foot is longer and more slender, with a longer and thicker middle toe, than in the older eocene type-genus, whence the generic name *Anchitherium* applied to them by von Meyer.‡ It is interesting, also, to find that the transitional character is further marked by the smaller relative size of the first premolar, whereby *Anchitherium* intervenes, as in the modification of the feet, between the *Palæotherium* and *Hipparion*.

Thus amply and satisfactorily has been fulfilled Cuvier's requisition of 1821:—'Entre le palæotherium et les espèces d'aujourd'hui l'on devrait decouvrir quelques formes intermédiaries.' How, then, is the origin of these intermediate gradations to be interpreted? One may first remark, that as *Palæotherium*, *Paloplotherium*, *Anchitherium*, *Hipparion* and *Equus*, differ from each other in a greater degree than do the Horse, Zebra, and Ass, the difficulty of interbreeding would be greater, and the probability of fertility less, supposing those extinct

* This modification, as the *Palæotherium ovinum* Aymard, began to be shown at the upper eocene at Velay, e. g., ere *Palæotherium* proper had passed away. (Bulletin du Congrès Scientifique de France tenu à Puy, 1855.)

† Also in the upper eocene of the Basin of the Garonne, with *Acerotherium*.

‡ *Anchitherium* occurs, also, in the 'marine-molasse,' or lower miocene, of St. Genies, Languedoc.

genera to have co-existed. One cannot doubt, also, that every well-marked species of these genera paired within itself, and that they exemplified respectively the character of a 'group of individuals descended from common parents, or from such as resembled them as closely as they resembled each other.' They did not, however, exist as species, during the same periods of time, far less so 'from the beginning of things.' The single-hoofed Horse-family cannot be traced further back than the pliocene tertiary period: the tridactyle equine species have not been found in strata earlier than miocene, and disappear in the upper eocene: the heavier-bodied shorter-legged species with three functional hoofs to each foot belong to upper and middle eocenes. Furthermore, in the oldest eocene (London clay, super-cretaceous Conglomerates and Plastic clay at Meudon, Paris,) we get evidence of Ungulates (*Pliolophus*, *Hyracotherium*, *Coryphodon*,) in which the perisso- and artio-dactyle characters were less differentiated than in *Palæotherium* and *Anoplotherium*, affording additional significant evidence of progressive departure from generalized type. Thus, the succession in time accords with the gradational modifications by which *Palæotherium* is linked on to *Equus*.

With this additional knowledge the question, 'whether actual races may not be modifications of those ancient races which are exemplified by fossil remains?' presents itself under very different conditions from those under which it passed before the minds of Cuvier* and the Academicians of 1830. If the alternative—species by miracle or by law?—be applied to *Palæotherium*, *Paloplotherium*, *Anchitherium*, *Hipparion*, *Equus*, I accept the latter, without misgiving, and recognize such law as continuously operative throughout tertiary time.

In respect to its mode of operation, we may suppose Lamarck to say, 'as the surface of the earth consolidated, the larger and more produced mid-hoof of the old three-toed Pachyderms took a greater share in sustaining the animal's weight; and, more blood being required to meet the greater demand of the more active middle-toe, it grew; whilst the side-toes, losing their share of nourishment and becoming more and more withdrawn from use, shrank;' and so on, according to the hardening of the ground, until only the hidden rudiments of metapodials remained and one hoof became maximized for all the work. Mr. Darwin, I conceive, would modify this, like other Lamarckian instances, by saying that some individuals of *Palæotherium* happening to be born with a larger and longer middle-toe, and with shorter and smaller side-toes, such variety was better

* Pourquoi les races actuelles, me dirait-on, ne seraient-elles pas des modifications de ces races anciennes que l'on trouve parmi les fossiles?—CXXXIX, i, p. lvii.

adapted to prevailing altered conditions of the earth's surface than the parental form; and so on, until finally the extreme equine modifications of foot came to be 'naturally selected.' But the hypotheses of appetency and volition, as of natural selection, are less applicable, less intelligible, in connection with the changes in the structure and proportion of the molar series of teeth, which we have seen also to be gradational from *Palæotherium* to *Equus*, fig. 614.

Any modification of Geoffroy's 'ambient medium,' affecting the density of the soil might so far relate to the changes of limb-structure, as that a foot with a pair of small hoofs dangling by the sides of the large one, like those behind the cloven hoof of the ox, would cause the foot of the Hipparion, e. g., and *à fortiori* the broader based three-hoofed foot of the Palæothere, to sink less deeply into swampy soil, and be more easily withdrawn, than the more concentratively simplified and specialized foot of the Horse.*

Rhinoceroses and Zebras, however, tread together the arid plains of Africa in the present day: and the Horse has multiplied in that half of America where two or more kinds of Tapir still exist. That the continents of the eocene or miocene periods were less diversified in respect of swamp and sward, pampas or desert, than those of the pliocene period, has no support from observation or analogy.

Assuming, then, that *Palæotherium* did ultimately become *Equus*, I gain no conception of the operation of the effective force by personifying as 'Nature' the aggregate of beings which compose the universe, or the laws which govern these beings, by giving to my personification an attribute which can properly be predicated only of intelligence, and by saying, 'Nature has selected the mid-hoof and rejected the others.'

As some paragraphs in my 'Preface' have been misconceived,† I must further observe, to put my meaning beyond doubt, that, to say that *Palæotherium* has graduated into *Equus* by 'Natural Selection' is an explanation of the process of the same kind and value as that which has been proffered of the mystery of 'secretion.' For example, a particular mass of matter in a living animal takes certain elements out of the blood and rejects them as 'bile.' Attributes were given to the liver which can only be predicated of the whole animal: the 'appetency' of the liver, it was said, was for the elements of

* xvii′, p. 397.

† Referring to my 'Anatomy of Vertebrates,' in the fourth edition of the 'Origin of species by Natural Selection,' &c., the author asserts that 'he' (Professor Owen) 'at the same time admits that Natural Selection may have done something toward this end.' Mr. Darwin does not quote the passage or refer to the page on which he founds his assertion.—ccxiii″ (1866), Histor. Pref., p. xviii.

bile, and 'biliosity' or the 'hepatic sensation' guided the gland to their selection.*

Such figurative language, I need not say, explains absolutely nothing of the nature of bilification. One's surprise is that 'tropes' and 'personified acts' should not have died out, as explanatory devices, with the 'archeus faber,' the 'nisus formativus,' and other self-deceiving, world-beguiling simulacra of science, with the last century; and that a resuscitation should have had any success in the present. It is of interest, mainly, as illustrating the 'alternation of generations.'

What, then, are the facts on which any reasonable or intelligible conception may be formed of the mode of operation of the derivative law exemplified in the series linking on *Palæotherium* to *Equus?* A very significant one is the following:—A modern horse occasionally comes into the world with the supplementary ancestral hoofs. From Valerius Maximus,† who attributes the variety to *Bucephalus*, downward, such 'polydactyle' horses have been noted as monsters and marvels. In one of the latest examples,‡ the inner splint-bone, answering to the second metacarpal of the pentadactyle foot, supported phalanges and a terminal hoof, in position and proportion to the middle hoof, resembling the corresponding one in *Hipparion*, fig. 614, II.

In relation to actual horses such specimens figure as 'monstra per excessum;'§ but, in relation to miocene horses, they would be normal, and those of the present day would exemplify 'monstra per defectum.' The mother of a 'monstrous' tridactyle colt might repeat the anomaly and bring forth a tridactyle 'filly'; just as, at San Salvador, the parents of a family of six had two of the series born with defective brain and of dwarf size: they were 'male' and 'female;' and these strange little idiots are exhibited as 'Aztecs.' The pairing of the horses with the metapodials bearing, according to type, phalanges and hoofs, might restore the race of hipparions.

Now, the fact suggesting such possibility teaches that the change would be sudden and considerable: it opposes the idea that species are transmuted by minute and slow degrees. It

* CCCXXVIII", vol. i, p. 268, and *passim*.

† 'Exemplorum memorabilium Libri novem, &c. (De rebus mirificis.)'

‡ CCCIV", p. 55, Pl. 1.

§ Two such examples are described in LII, vol. ii, and one in CCCV", p. 224, in which the left fore-foot had three subequal hoofs, and the right fore-foot two hoofs. But the application of an instructive and rightly discerned relation may be travestied and exaggerated: the two-tailed Lizard and the double-headed snake do not reproduce to view normal ancestral forms. The essentially single mid-toe (fig. 193, iii) of the horse, occasionally bifid and terminated by a pair of ill-shapen hoofs, lends no support to the idea of the digit (iii) being homologous with the so-called cloven hoof (really the digits iii and iv, ib.) of Ruminants. It is a malformation akin to that of the partially double digit of the Dorking fowl.

also shows that a species might originate independently of the operation of any external influence; that change of structure would precede that of use and habit; that appetency, impulse, ambient medium, fortuitous fitness of surrounding circumstances, or a personified 'selecting Nature,' would have had no share in the transmutative act.

There is, however, one relation which I cannot shut out, for I hold it as strongly as when I explained it, and endeavored to impress it upon the audience at my lectures of 1857: it is the fitness of the organization of the Horse and Ass for the needs of mankind, and the coincidence of the origin of Ungulates having equine modifications of the perissodactyle structure with the period immediately preceding, or coincident with, the earliest evidence of the Human Race.

Of all the quadrupedal servants of Man none have proved of more value to him, in peace or war, than the horse: none have coöperated with the advanced races more influentially in Man's destined mastery over the earth and its lower denizens. In all the modifications of the old palæotherian type to this end, the horse has acquired nobler proportions and higher faculties, more strength, more speed, with amenability to bit. No one can enter the 'saddling ground' at Epsom, before the start for the 'Derby,' without feeling that the glossy-coated, proudly-stepping creatures led out before him are the most perfect and beautiful of quadrupeds. As such, I believe the Horse to have been predestined and prepared for Man. It may be weakness; but if so, it is a glorious one, to discern, however dimly, across our finite prison-wall, evidence of the 'Divinity that shapes our ends,' abuse the means as we may.

Thus, at the acquisition of facts adequate to test the moot question of links between past and present species, as at the close of that other series of researches proving the 'skeleton of all Vertebrates, and even of Man, to be the harmonized sum of a series of essentially similar segments,'* I have been led to recognize species as exemplifying the continuous operation of natural law, or secondary cause; and that, not only successively, but progressively; from the first embodiment of the Vertebrate idea under its old Ichthyic vestment until it became arrayed in the glorious garb of the Human form.'†

* CXLI, p. 119.

† Ib., p. 86. Even in his partial quotation from my work of 1849, the author of CCXIII'' (4th Ed. 1866) might have seen ground for apologizing for his preposterous assertion, in 1859:—that 'Professor Owen maintained, often vehemently, the immutability of species' (p. 310), and for the question, as preposterous and unworthy: 'Does he really believe that at innumerable periods in the earth's history elemental atoms have been commanded suddenly to flash into living tissues?' (Ib., Ed. 1859, p. 483. In the Ed. of 1860, p. 111, the imputation is tacitly abandoned). The significance of the concluding paragraphs of CXLI was plain enough to BADEN POWELL, CCCXXXIII'', p. 401 (1855), and drew down on me the hard epithets with which Theology usually assails the inbringer of unwelcome light, CII', p. 61.

The series of observations on the Ungulate group of Mammals yields insight, as above explained, into the mode of operation of the secondary law; and gives evidence of the amount of geological time intervening between the introduction and disappearance of generic and subgeneric modifications. According to the analogy of the mammalian *Hipparion* and *Equus*, we may expect the corresponding precedent form of the Papuan of the well-wooded and richly fruited islands representing a departed tropical or subtropical continent, to be exemplified by fossils in formations not earlier than middle tertiary. All species coëxisting with the actual specific form of *Homo* will, with him, be immutable, or mutable only as he may be. To name such species, after comparing and determining their specific characters, will continue to be the Zoologist's staple task as long as his own specific intellectual character remains unchanged (Pref., p. xxxvi). To suppose that coëxisting differentiations and specializations, such as *Equus* and *Rhinoceros*, or either of these and *Tapirus*, which have diverged to generic distinctions from an antecedent common form, to be transmutable one into another, would be as unscientific, not to say absurd, as the idea, which has been bolstered up by so many questionable illustrations, and foisted upon poor 'working men,' of their derivation from a Gorilla!

§ 425. *Extinction, cataclysmal or regulated?*—If, in place of recognizing the series of the above-cited Perissodactyles as evidencing (preordained) departures from parental type, probably sudden and seemingly monstrous, but adapting the progeny inheriting such modifications to higher purposes, the theological notion be retained, and the species of Palæothere, Paloplothere, Anchithere, Hipparion, and Horse, be severally deemed due to remotely and successively repeated acts of direct creation, one is concomitantly led to suppose the successive going out of such species to have been as miraculous as their coming in. The destruction of one creation is the logical preordinance to a recurrence of 'genesis.' This nexus of ideas was too close not to have swayed with Cuvier: accordingly, in his famous 'Discours sur les Révolutions de la Surface du Globe,' we have a section of 'Preuves que ces Révolutions ont été nombreuses,'* and another section of 'Preuves que ces Révolutions ont été subites.'† Continued observations of Geologists, while establishing the fact of successive changes, have filled up the seeming chasms between such supposed 'revolutions,' as the discoveries of Paleontologists have supplied the links between the species held to have perished by the cataclysms. Each successive parcel of geological truth has tended to dissipate the belief in the un-

* cccxx", p. 5. † Ib., p. 8.

usually sudden and violent nature of the changes recognizable in the earth's surface. In specially directing my attention to this moot point, whilst engaged in investigations of fossil remains, and in the reconstruction of the species to which they belonged, I was, at length, led to recognize one cause of extinction as being due to defeat in the 'contest which as a living organized whole, the individual of each species had to maintain against the surrounding agencies which might militate against its existence.' (Pref., p. xxxiv.) This principle has received a large and most instructive accession of illustrations from the extensive knowledge and devoted labors of Charles Darwin: but he aims to apply it not only to the extinction but the origin of species.

Although I fail to recognize proof of the latter bearing of the 'battle of life,' the concurrence of so much evidence in favor of 'extinction by law' is, in like measure, corroborative of the truth of the ascription of the origin of species to a secondary cause.

§ 426. *How works the Derivative Law?*—The guesses made by those who have given the rein to the imaginative faculty in attempts to explain the mode of operation of the derivative law have mainly proved repellent to its study, and have raised the chief obstacles to its acceptance, by affording the most favorable opportunities of telling argument and caustic criticism to opponents of any recognition of such law in the abstract. Thus, De Maillet's conception of the conditions of transmutation invited Cuvier's crushing exposition of its absurdity, which fell with the full weight of his great anatomical knowledge. Lamarck gave occasion to many similar confutations, applied not always in good faith, and often by men without any anatomical or physiological qualifications for such criticism, to discredit veritable evidences of the operation of a secondary creative law. Subjoined, for example, is his hypothesis of the origin of the human species,* which, with similar illustrations from the web-footed, hoofed, and long-necked ruminant mammalia, have afforded topics of easy ridicule. So Lyell, asserting that, 'orangs had been tamed by the savages of Borneo, and made to climb lofty trees and bring down the fruit,'†

* 'Effectivement, si une race quelconque de *quadrumanes*, surtout la plus perfectionée d'entre elles, perdoit, par la nécessité des circonstances ou par quelqu'autre cause, l'habitude de grimper sur les arbres et d'en empoigner les branches avec les pieds, comme avec les mains, pour s'y accrocher; et si les individus de cette race, pendant une suite de générations, étoient forcés de ne servir de leurs pieds que pour marcher, et cessoient d'employer leurs mains commes des pieds; il n'est douteux, d'après les observations exposées dans le chapitre précédant, que les quadrumanes ne fussent à la fin transformés en *bimanes*, et que les pouces de leurs pieds ne cessassent d'être écartés des doigts, ces pieds ne leur servant plus qu'à marcher.'—CCXCVIII", i, p. 349.

† CCC", Ed. 1835, vol. ii, p. 463.

proceeds :—'It is for the Lamarckians to explain how it happens that these same savages of Borneo have not themselves acquired, by dint of longing, for many generations, for the power of climbing trees, the elongated arms of the orang, or even the prehensile tails of some American monkeys. Instead of being reduced to the necessity of subjugating stubborn and untractable brutes, we should naturally have anticipated "that their wants would have excited them to efforts, and that continued efforts would have given rise to new organs :" or, rather, to the reacquisition of organs, which in a manner irreconcilable with the principle of the "progressive" system, have grown obsolete in tribes of men which have such constant need of them.'*

An anatomist and physiologist competent to judge of the stable grounds of a derivative origin of species—unity of plan, geological epochs, successive species therein,—truly set forth by the great and philosophic naturalist, would have referred to him, bearing calmly and nobly an old age of blindness and poverty, in a more worthy spirit. From one destitute of qualifications for grappling with the difficulties of this profound genetic problem in physiology, silence would have been blameless. Vituperative condemnation by such a one of a given phase or an untenable ground of that problem is of no greater value than his extravagant commendation, with as little capacity for comprehending its weakness, of a subsequent attempt toward its solution.

Some of Lamarck's characteristic and assailable illustrations have indeed been adopted and further developed :—'Ceux des mammifères aquatiques qui contractèrent l'habitude de ne jamais sortir des eaux, et seulement de venir respirer à leur surface, donnèrent probablement lieu aux différens *Cetacées*. En effet, depuis l'énorme quantité de temps que ces animaux vivent dans le sein des mers, ne se servant jamais de leurs pieds postérieurs pour saisir les objets, ces pieds non employés ont tout-à-fait disparu, ainsi que leurs os, et même le bassin qui leur servoit de soutien et d'attache.† As a fact, however, so much of the pelvis has been preserved in *Cetacea* as serves to give origin to certain muscles of the genitals : and, in the mysticete whale, even a rudiment of the attached limb remains (vol. ii, fig. 159, $_{63-66}$). But beside the influence of habitual sojourn in water, Mr. Darwin adds another consideration to account for the enormous head in *Cetacea* :—'In North America the black bear was seen by Hearne swimming for hours with widely open mouth, thus catching, almost like a whale, insects in the water.‡ I see no difficulty in a race of bears

* Ib., p. 464. † CCXCVIII'', ii, p. 461. ‡ CCXIII'', p. 184, Ed. 1.

being rendered by Natural Selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale.'*

The idea which Mr. Darwin persuades himself that he originated in addition to Lamarck's 'influence des circonstances sur les actions et les habitudes des animaux et de celle des actions et des habitudes de ces corps vivans, comme causes qui modifient leur organization et leurs parties' is most intelligibly illustrated in the paper in which he first communicated his views to the Linnæan Society. It is by 'an imaginary example from changes in progress on an island':—'Let the organization of a canine animal which preyed chiefly on rabbits, but sometimes on hares, become slightly plastic; let these same changes cause the number of rabbits very slowly to decrease, and the number of hares to increase: the effect of this would be that the fox or dog would be driven to try to catch more hares; his organization, however, being slightly plastic, those individuals with the lightest forms, longest limbs, and best eyesight, let the differences be ever so small, would be slightly favored, and would tend to live longer, and to survive during that time of the year when food was scarcest; they would also rear more young, which would tend to inherit those slight peculiarities. The less fleet ones would be rigidly destroyed. I can see no more reason to doubt that these causes in a thousand generations would produce a marked effect, and adapt the form of the fox or dog to the catching of hares instead of rabbits, than that greyhounds can be improved by selection and careful breeding.'† So Geoffroy Saint-Hilaire also wrote:—'Si ces modifications amènent des effets nuisibles, les animaux qui les éprouvent cessent d'exister, pour être remplacés par d'autres, avec des formes un peu changées, et changées à la convenance des nouvelles circonstances.'‡

The modifications on which Geoffroy Saint-Hilaire laid chief stress were those assumed to have affected the ambient medium, the mode of operation of which in the origin of species he thus exemplifies:—'Mon Mémoire, traitant de l'influence des milieux ambians pour modifier les formes animales, montre comment la quantité décroissante de l'oxygène, relativement aux autres composans de l'atmosphère, a pu forcer les surfaces cutanées des embryons, premier et principal siége des actes respiratoires, à s'ouvrir davantage, à gagner, dans une raison inverse du volume existant de l'oxygène, plus de profondeur, au moyen de plus larges anfractuosités dans le tissu cellulaire,

* This conclusion of the passage is omitted in later editions.

† CCCI, p. 49. But see the remarks on this in CLXXX, p. 434, and CII', p. 65.

‡ CCXCIX'', p. 79.

et à acquérir, par un accroissement dans l'intensité des effets, de plus en plus, le caractère d'ampoules et décidément de trachées, jusqua' à ce qu'enfin survienne dans le thorax une concentration des sinus respiratoires, et des arrangements de structure pour l'isolement des poches ou théâtres de respiration, appelés, suivant leurs qualités conditionnelles, *poumons* ou *branchies*.'—CCXCVII", p. 82.

One should not be dealing fairly with this exposition of transmutative conditions if we were to take its terms in their literal or usual acceptation ; else, the obvious objection that embryos are shut out from the influence of the atmosphere until their lungs are prepared for it, at once suggests itself. I assume, therefore, that the term is used, metaphorically, to signify the low and early embryo-like forms of living things. But it may then be remarked that if speculation be permitted on possible changes in the constitution of the atmosphere of this planet, during past geological æons, it is more probable that the proportion of the carbonic acid has been reduced than that of the oxygen. The prevalence of remains of cold-blooded slow-breathers in palæozoic and older mesozoic strata has more than once suggested such relation to the 'ambient medium.' I repeat, however, that the sole consequence of vague generalities, or figurative impersonations, propounded to show how transmutation may go on, has been to prejudice calm and sound judgments against any acceptance of, or favor toward, the grounds of a belief in secondary creational law. I have elsewhere tested the ideas of Lamarck and Darwin as to the mode of transmutation, by reference to the species *Chiromys Madagascariensis* :* I will now apply them, together with Geoffroy's, to another and lower degree of life.

What spectacle can be more beautiful, striking and suggestive than that of the inhabitants of the calm expanse of water of an atoll, encircled by its vast ring of coral rock ! Leaving the bright-tinted Chœtodonts, the Scari with adamantine jaws, the Holothurians and other locomotive frequenters of the calcareous basin out of the question, and restricting the test to the species cemented or otherwise confined to its area : we may first ask :—

Were the elements of the coriaceous and of the softer contractile and secreting tissues of the coral-polyp suddenly combined and disposed so as to form the body-wall, inverted gastric-bag, produced tentacles, intermediate laminæ, generative plaits, vesicles and threads, with outer folds in arrangement and numbers such as to secrete the laminate calcareous polyp cell? Was the creature, so miraculously constituted, at the same

* CII", pp. 64–66.

time endowed with generative faculties to multiply and reproduce its kind for all time; the creative act henceforth and thereafter being dispensed with? Accepting, with the theologian, this view, it must then be applied to each of the more or less closely allied species associated in the same coral workhouse. The origin of such species thus dates back to the beginning of life on the globe.* The first created coral-polyp included, potentially, the germs of its successors throughout all time.

Observation, however, shows that the species of existing *Anthozoa* cannot be traced very far back: those with a flexible, or with a branched, calcareous axis began only at the tertiary period; and, of the genera of eocene lamellate or stony corals, all the species are extinct, and have been superseded in their grand and useful operations by those now forming reefs and atolls. As we extend our researches back in time we find generic and family types of coral-polyps passing away: the prevalent pattern of stellate cups of rays of *six* or its multiples, has superseded a simpler pattern of *four* or its multiples. Of the *Cyathophyllidæ* of the palæozoic reefs which present a quadripartite character of their plaited polyp-cells, not one such species now exists, or has been observed in any formation later than lower green-sand. Moreover, the filling up of abandoned cells in the course of growth of the polypary becomes changed from a more complex to a more simple method, as we recede in time in pursuing our comparisons.†

With this generalized result of observation of reef-building polyps we return to the initial question in a frame of mind inevitably other than that in which the creation of a coral-island is pondered on by one ignorant of the geological history of the class engaged in its construction. Was direct creation, after the dying out of its result as a 'rugose coral,' repeated to constitute the succeeding and superseding 'tabulate coral'? Must we, also, invoke the miraculous power to initiate every distinct species of both *Rugosa* and *Tabulata*? These grand old groups have had their day and are utterly gone. When we endeavor to conceive or realize such mode of origin, not of them only, but of their manifold successors, the miracle, by the very multiplication of its manifestations, becomes incredible—inconsistent with any worthy conception of an all-seeing, all-provident Omnipotence! It is not above, but against, rea-

* I leave out of the question the subsequent lethal influence of the heavy and continuous rain added to the ocean in order to raise it above the highest mountains, according to the biblical flood.

† CLXXX, pp. 23–28.

son ; and I may assume the special primary creative hypothesis of the successive and coexisting species of *Anthozoa* to be not now held by the scientific naturalist.

Let us then test the propounded explanations of their origin by secondary law. That of 'appetency' subsides from the impotency of a coral-polyp to exercise volition. The weak point of Lamarck's creative machinery is its limited applicability, viz: to creatures high enough in the scale to be able to 'want to do something:' for the determined laws of the 'reflex function' in the physiology of the nervous system and the necessity of the superadded cerebral mass for true sensation rigorously fix the limits of volitional faculties.

We pass then to considerations of the 'ambient medium' and 'natural selection.' We have no evidence that the fabricators of the coral-reef of Wenlock-edge, or of those skirting the Cambrian slates and Devonshire 'killas,' or of those in the lofty limestone cliffs of Cheddar, worked in an ocean otherwise constituted than the present. What conceivable character of sea or of the air dissolved or diffused therein could have changed the loose aggregation of the individuals of composite *Rugosa* into the close combination, with intercommunicating pores, of those of the composite *Tabulata*? Or what possible external influence could have transmuted the comparatively simple massive mode of growth or deposition of carbonate of lime common to both *Rugosa* and *Tabulata* into the light and complex character of the polyparies of most existing lamelliferous *Anthozoa*? In the first mode the old polyp-cell is successively partitioned off from the one in occupation by floor after floor crossing the cavity: in the other, radiating vertical partitions alone occupy the deserted cell and extend uninterruptedly from its bottom or beginning to the superficial inhabited chamber. The quadripartite pattern of the plaited cup of the palæozoic coral has changed into the sexpartite disposition of the radiating lamellæ of the polyp-cells of tertiary and modern corals. But personifying the fact of such transmutations by the term 'natural selection' gives no more insight into the manner of the operations than we learn of that of the budding out of a new leg in a maimed newt, by being told that it is done by the 'nisus formativus' or by 'pangenesis'! Even were there evidence of changes in the composition of the atmosphere, their 'modus operandi' in effecting such structural differences would not be more conceivable.

I do not believe that a sexpartite type of coral was miraculously created to supersede a quadripartite one. If the grounds are good for admitting the continuous operation of a secondary cause of the specific forms of Vertebrate life, *à fortiori* it is

admissible in the lower sphere of Radiate life. It is consistent with facts that a quadripartite coral might bud out, or otherwise generate, a variety with a greater number of radiating laminæ. Some varieties, like those expressed by the modern generic terms *Porites*, *Millepora*, especially the *M. complanata*, with its strong vertical plates, were better adapted to bear the brunt of the breakers, and flourish in the surf, under the protection of the coating Nullipore. But to how small an exception is this relation applicable! Of the 120 kinds of coral enumerated by Ehrenberg in the Red Sea,* 100, at least, exist under the same conditions. The majority of species, originating in uncalled-for, unstimulated, unselected departures from parental structure, establish themselves and flourish independently of external influences. All classes of animals exemplify this independence: the Cetaceans, under an extraordinary and nicely graduated range of generic and specific modifications; and the same may be said of most Fishes.†

So, being unable to accept the volitional hypothesis, or that of impulse from within, or the selective force exerted by outward circumstances, I deem an innate tendency to deviate from parental type, operating through periods of adequate duration, to be the most probable nature, or way of operation, of the secondary law, whereby species have been derived one from the other.

It operates, and has operated, in the surface-zones where the chambered cephalopods floated, and at the depths where the brachiopods were anchored, as in the more defined theatre in which the various polyps of the coral reef display their diversities of color, size, shape, and structure, independently of outward influences. This tendency, moreover, is not exemplified in the ratio of the number, variety, or force of conceivable 'selective' surrounding influences, but is directly as the simplicity of the organism. In the *Foraminifera*, e. g., it is manifested in such degree that as many as fifteen genera defined by one given to—

> Intrigue with the specious chaos, and dispart
> Its most ambiguous atoms with sure art;
> Define their pettish limits, and estrange
> Their points of contact and swift counterchange,

have been found by his followers to be but varieties of a single type; and even this, too inconstant to come under the definition of a species given in p. 7. The departure from parental form, producing the beautiful varieties of perforate and imperforate Rhizopods, and which exemplify each group, respectively, under the Lagenine, Nummulinine, Globigerine, or

* CCCXIX″, p. 46. † XCIX′, p. 44.

under the Gromiine, Milioline, and Lituoline types, has effected its ends independently of inner volitions or of outer selections. Certain incrusting forms seem by the presence of siliceous spicula to have been derived from sponges; but no explanation presents itself for such transitional changes, save the fact of anomalous, monstrous births—as these varieties, and the whole assemblage of alternate-generative phenomena, would be called 'in high life.'

According to my derivative hypothesis, a change takes place first in the structure of the animal, and this when sufficiently advanced, may lead to modifications of habits. But we have no evidence that the observed amount of change in *Porifera*, *Foraminifera* and *Anthozoa*, &c., has been attended with any change in the way or power in which they extract from their ambient medium, and precipitate, silex and carbonate of lime, or in the performance of any other vital function. As species rise in the scale, the concomitant change of structure can and does lead to change of habits. But species owe as little to the accidental concurrence of environing circumstances as Kosmos depends on a fortuitous concourse of atoms. A purposive route of development and change, of correlation and interdependence, manifesting intelligent Will, is as determinable in the succession of races as in the development and organization of the individual. Generations do not vary accidentally, in any and every direction; but in preordained, definite, and correlated courses.

If the survey of a series of siliceous polycystins and diatoms, of zoophytes, of brachiopods, of ammonites, excites pleasure by their beauty, and raises worship of the Power manifesting itself in such inconceivable and exhaustless variety, I accept the relation as one designed, and in His due time, fulfilled:—

> To doubt the fairness were to want an eye;
> To doubt the goodness were to want a heart!

'Derivation' holds that every species changes, in time, by virtue of inherent tendencies thereto. 'Natural Selection' holds that no such change can take place without the influence of altered external circumstances educing or selecting such change.

'Derivation' sees among the effects of the innate tendency to change, irrespective of altered surrounding circumstances, a manifestation of creative power in the variety and beauty of the results: and, in the ultimate forthcoming of a being susceptible of appreciating such beauty, evidence of the preordaining of such relation of power to the appreciation. 'Natural Selection' acknowledges that if ornament or beauty, in

itself, should be a purpose in creation, it would be absolutely fatal to it as a hypothesis.

'Natural Selection' sees grandeur in the "view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one :"* 'Derivation' sees, therein, a narrow invocation of a special miracle and an unworthy limitation of creative power, the grandeur of which is manifested daily, hourly, in calling into life many forms, by conversion of physical and chemical into vital modes of force, under as many diversified conditions of the requisite elements to be so combined.

'Natural Selection' leaves the subsequent origin and succession of species to the fortuitous concurrence of outward conditions: 'Derivation' recognises a purpose in the defined and preordained course, due to innate capacity or power of change, by which nomogenously-created protozoa have risen to the higher forms of plants and animals.

The hypothesis of 'derivation' rests upon conclusions from four great series of inductively established facts, together with a probable result of facts of a fifth class: the hypothesis of 'natural selection' totters on the extension of a conjectural condition, explanatory of extinction to the origination of species, inapplicable in that extension to the majority of organisms, and not known or observed to apply to the origin of any species.

§ 427. *Epigenesis or Evolution ?*—The derivative origin of species, then, being at present the most admissible one, and the retrospective survey of such species showing convergence, as time recedes, to more simplified or generalized organizations, analogous to von Baer's law of individual development, the result to which the suggested train of thought inevitably leads is very analogous in each instance. If to Kosmos or the mundane system has been allotted powers equivalent to the development of the several grades of life, may not the demonstrated series of conversions of force have also included that into the vital form ?

In the last century, physiologists were divided as to the principle guiding the work of organic development.

The 'evolutionists' contended that the new being pre-existed in a complete state of formation needing only to be vivified by impregnation in order to commence the series of expansions, or disencasings, culminating in the independent individual.

The 'epigenesists' held that both the germ and its subsequent organs were built up of juxtaposed molecules according

* CCXIII", Ed. 1860, p. 490.

to the operation of a developmental force, or 'nisus formativus.'

Haller maintained the principle of 'evolution,' Buffon that of 'epigenesis.' Hunter, who surpassed all his contemporaries in observations on the formation of the chick, 'thought he could see both principles at work, together with a third.' However, as he limited the 'pre-existing entities' to 'the materia vitæ universalis' and the 'absorbent faculty,' he would now be classed with the 'epigenesists.' For, he reckoned among the parts newly built up, not evolved, 'the brain and heart, with their appendages, the nerves and vessels, and so on of all the other parts of the body which we do not find at first.'* His third principle is merely a modification of epigenesis, viz: 'change in form and action of pre-existing parts.'

At the present day the question may seem hardly worth the paper on which it is referred to.† Nevertheless, 'pre-existence of germs' and 'evolution' are logically inseparable from the idea of the origin of species by primary miraculously created individuals. Cuvier, therefore, maintained both, as firmly as did Haller.‡ It is, perhaps, one of the most remarkable instances of the degree in which a favorite theory may render us blind to facts which are opposed to our prepossessions. Hunter's demonstrations of the epigenetic development of the blastoderm and initial parts of the chick§ were not known to Cuvier; but the analogous ones of Wolff‖ he had studied. To the phenomena of the blood-lakes and their union in order to constitute the 'circulus vasculosus' of the vitellicle, Cuvier opposes the following remark:—'Mais il faut nécessairement admettre qu'il y avait une pré-existence de quelques chemins pour les pointes rouges; car en virtue de quelle force la figure veineuse serait-elle toujours composée des mêmes vaisseaux ayant la même direction? Comment ces vaisseaux aboutiraient-ils toujours au même point pour former un cœur? Tous ces phénomènes ne sont intelligibles qu'autant qu'on admet quelque pré-existence.'¶

Haller, who had made some good observations on embryonal development, confessed that there was a stage in that of the chick in which the 'intestinal canal was not visible;' he would

* xx, vol. v, p. xiv.

† The encasement or imboxing ('emboîtement') of germs was deemed, a century or more ago, to receive support from the evolution of buds and other parts of plants, and from Swammerdam's discoveries in the chrysalis, not only of the parts which afterwards form the butterfly, as wings, antennæ, &c., but also of the eggs which were to be laid in that phase of life. Bonnet drew an inference in favor of the same view from his discovery of the numerous successive generations of *Aphides* which might be impregnated by a single copulation. (See, however, cxlii,' pp. 27, 39)

‡ xxviii''.

‖ cccvi''.

§ xx, vol. v, Pls. lxviii,–lxxviii.

¶ cccvii'', tom. iv, p. 236.

not admit, however, that it was not formed, or that it did not pre-exist; but affirmed that it was too minute to be perceived: not until the head and limb-buds of the chick appeared, was the intestine visibly 'evolved.'*

To the beautiful demonstration of the steps in the successive building up and molding of the intestinal canal, out of the 'mucous layer' of the blastoderm, Cuvier objects:—

'Mais quand il serait vrai que l'intestin se forme comme Wolff croyait l'avoir observé, il n'en résulterait aucune preuve en faveur de l'épigénèse; car le nombril, par lequel l'embryon tient son placenta, est d'abord tout aussi large que l'animal lui-même; c'est en enveloppant la portion du jaune qui doit rester dans l'intérieur, que la peau finit par rétrécir de plus en plus cette ouverture, qui primitivement n'en était pas une, et par la réduire à l'ombilic tel qu'on le voit dans le poulet ou dans l'enfant naissant.'†

Geoffroy contended that the dogma of 'pre-existence of germs' owed its origin to a metaphysical explanation of ill-observed phenomena. To admit that a germ included within itself all the forms, in miniature, which were afterward to be manifested, and to develop such theory by a matter so indefinable, was to multiply, at will, the most gratuitous suppositions.‡

His opponent's passages, above quoted, in defense of a doctrine now deemed by embryologists to be dead and buried, have hardly other than historical interest; and I should not have recalled them, or their subject, were it not that ghosts of 'pre-existence' and 'evolution' still haunt some chambers of the physiological mansion, and even exercise, to many, perhaps an unsuspected sway over certain biological problems.

Although in the Debates of 1830, the question of 'Pre-existence of Germs,' was the sole one in which, as applied to Embryogeny, I held with Geoffroy Saint-Hilaire, I remained the thrall of that dogma in regard to the origin of single-celled organisms, whether in or out of body.§ Every result of formifaction I believed, with most physiologists, to be the genetic outcome of a pre-existing 'cell.' The first was due to miraculous interposition and suspension of ordinary laws; it contained, potentially, all future possible cells. Cell-development exemplified evolution of pre-existing germs, the progeny of the primary cell. They propagated themselves by self-division, or by 'proliferation' of minute granules or atoms, which, when

* "Partes animalis non noviter formantur, sed transeunt ex statu obscuro in conspicuum."—XXVIII", tom. viii, sectio 2da. p. 150–156. Also 'Mémoire II, sur la formation du Poulet,' p. 182.

† CCCVII", tom. iv, p. 277. ‡ Anat. Philos., vol. ii, p. 280.

§ CCXLIX, CXLII.

properly nourished, again multiplied by self-division, and grew to the likeness of the parent-cells.

Those who still hold by this rag of 'pre-existence of germs,' call all organic corpuscles or granules 'cell-gemmules,' and maintain that they are transmitted, sometimes becoming developed, sometimes lying dormant from generation to generation, independent, automonous, pre-existing from their primal miraculous creation, as descendants, like all higher forms of life 'of that one form of "Natural Selection" into which life was first breathed.' Darwin grafts upon this modification of the old evolutional dogma* his provisional hypothesis of 'Pangenesis.' (CCCVIII)".

In like manner the Evolutionists hold that every single-celled organism, torule, organic molecule, out of the body, arises from

* Studying under this belief the phenomena described in CXLII, I was led to regard all 'cells' or organic units concerned in development and repair as the progeny of the primary germ-cell in the ovarium of the mother, and to be in that sense 'derivative.' Save in the case of the hypothetical primordial created unit, such primary ovarian cell in the *Aphis* and all sexual organisms I regarded as impregnated. The derivative cells or organic units propagated themselves independently of direct sexual intercourse; but, that they should not be remotely or indirectly related to the act by which their seat, the developed organism, came to be,—in which organism, or its parthenogenetically propagated offspring, the 'cells' subsequently were formed,—was to me inconceivable on the then accepted hypothesis of 'pre-existence of germs' or 'omnis cellula e cellulâ.' Mr. Darwin, however, opposes to the above view the remark, "My gemmules" (=my germ-cells) "are supposed to be formed quite independently of sexual intercourse, by each separate cell or unit throughout the body." (CCCVIII", ii, p. 375.) Yet, his provisional hypothesis of 'pangenesis' assumes that they ('cells,' 'cell-gemmules,' 'units') "are transmitted from the parents to the offspring" ib.). But how so (in sexual species), save as the progeny or outcome of the primary impregnated germ-cell in the mother, whence all subsequent development and cell-generation radiated? Take any case in CCCVIII", which 'Pangenesis' is propounded to explain—and all the given instances of varieties, malformations, &c., are from sexual organisms—as e. g. 'when a stag is castrated the gemmules derived from the antlers of his progenitors quite fail to be developed.' (Ib., ii, p. 399): to each I should reply as to this case:—Such stag first existed as an impregnated unit in the oviducal ovum of the mother. By the 'spontaneous fission' or 'cleavage process' it must have existed as a mass of impregnated gemmules. Accepting the nonsense, that some of these gemmules were derived from the antlers of its parent, yet they are not less the progeny of the primary germ-cell which was formed within the ovarium of the female and was fertilized by the male. I fail, after every endeavor, to appreciate the 'fundamental difference' between Mr. Darwin's cell-hypothesis of 1868 and mine of 1849 (CXLII, p. 5—8). Both of them I now regard as fundamentally erroneous; in so far as they are absolutely based on 'pre-existence'—or 'omnis cellula,' &c. No doubt, many cells or organic units are derived from pre-existing cells (vol. i, p. 625): the phenomenon of the pale or granulated blood-cells which suggested to me, in 1838, the idea of the genetic mode of formation of the ordinary blood-discs, is a true phenomenon: but such mode of formation is subordinate to a wider law. Under given conditions matter in solution aggregates and shows form; if inorganic as 'crystal,' if organic as 'spherule': in the one the process is termed 'crystallization,' in the other 'formifaction.' If the large 'pale cell' was first filled by fluid holding organic matter in solution, the smaller granules or atoms it subsequently discharged might be the result of 'formifaction.' It is at least a more simple, and I believe truer, idea of their origin than that which ascribes such origin to a mysterious genetic act under the name of 'proliferation.'—(CCCVIII", vol. ii, p. 374.)

a pre-existent germ; and that such germs abound in the air, in the waters, or wherever any forms of living matter may happen to make their appearance.

§ 428. *Nomogeny* or Thaumatogeny?†* — The French Academy of Sciences was the field of discussion and debate, from 1861 to 1864, between the 'Evolutionists' holding the doctrine of primary life by miracle, and the 'Epigenesists,' who try to show that the phenomena are due to the operation of existing law. The analogy of the discussion between Pasteur and Pouchet, and that between Cuvier and Geoffroy, is curiously close. Beside the superiority in fact and argument, Pasteur, like Cuvier, had the advantage of subserving the prepossessions of the 'party of order' and the needs of theology. The justice of Jamin's summary,‡ awarding to the chemist the palm of superior care and skill both in devising and performing the experiments, and exposing the inferiority of the physiologist in polemical ability and coolness of argumentation, cannot be denied. Nevertheless, Pouchet is rapidly acquiring, in reference to the origin of monads, that position which Geoffroy Saint-Hilaire has taken in regard to the origin of species. It is a suggestive and instructive fact in the philosophy of mind and the history of progress.

Some rare instances, in every generation, are gifted with the faculty of discerning the light of truth through all obstruction: when its glimmer is of the feeblest their brain responsively vibrates through a barrier of beliefs, prepossessions, precise logic, across thickets of facts deemed to be rightly understood, athwart accepted 'laws' and principles, organized corps of the soldiers of science, public opinion, &c.; and these men never know when they are beaten and put out of court: happily, against all hindrance, they persist—*'e pur si muove.'*

Pasteur by an ingeniously devised apparatus,§ collected atoms in the atmosphere, and described and figured them as examples of 'organized corpuscles,' 'globules,' or the 'germs' of living things, there floating.‖ In a solution of organic matter, otherwise unfit for the development of life, the addition of some of these germs was followed by the appearance, in abundance, of its simple forms.

To the conclusion that the monads were the consequence, not merely the sequence, of the 'ensemencement,' it can be objected that the atmospheric atoms figured¶ are not like the

* *νόμος*, law, *γένω*, root of *γίγνομαι*, to 'become,' or come into being.
† *θαῦμα*, miracle, *γένω*.
‡ CCCXXXIV″, pp. 442, 443.
§ CCCIX″, p. 25, Pl. I, fig. 1.
‖ Ib., Pl. I, figs. 2–9.
¶ Ib., "quelques corpuscles organisées."—p. 28, Pl. I, figs. 2, 3, 4:—"tout-à-fait semblables à des germes d'organismes inférieures."—p. 37. Of the various well marked forms of ova or germs of lower organisms, I know not any recognizable in the figures above cited.

observed formified corpuscles by which bacteriums have been seen to be built up; and, that the chemical treatment to which they had been subject, in their extraction from the atmosphere, would be likely to destroy the vitality of fecund germs, if any were present. To the alleged absence of any organisms in the experiments which were calculated to exclude extraneous germs, and to unfit the infusion for the development of any it might contain, the graver objection applies, that the microscopic power employed by Pasteur in their search was insufficient. Dr. Child,* in experiments which seem to be as exclusive as Pasteur's, does obtain bacteriums, discoverable, at first, by a power of 1,500 diameters, and, once so seen, afterward recognizable by a power of 750 diameters: whereas Pasteur, in his quest, did not avail himself of a power exceeding 350 diameters, and consequently failed to detect the evidence of 'nomogeny,' under conditions as decisive as can be hoped in an attempt to prove a negative. Against 'panspermism,' or the dogma that animalcules of infusions come, invariably and exclusively, from pre-existing germs falling from the air, Pouchet records the results of experiments, conclusive or satisfactory from their simplicity and ease of repetition, and freedom from need of minute, ambiguous, manipulatory precautions.†

A glass tube containing a filtered infusion is placed in the middle of a glass dish containing the same infusion: this stands in a wider dish of water in which a bell-glass is placed covering the vessels with the infusion. At the end of four or five days the tube-infusion has a thick film abounding with ciliate infusoria: the dish-infusion has a thin reticulate film containing only bacteriums and other small non-ciliate 'microzoaires.' It is 'difficult to see how the germs of the one kind of creatures should have entered or become developed in the one vessel and entirely different kinds in the other.'‡

I refer the reader to CCCXII″ and CCCXXXV″ for further analysis of the grounds of the disputants, and proceed to remark, that the illustrations of the process of development of a *Paramecium*§ so closely resemble those of the ovarian ovum in Fish or Mammal, that either fig. 555 or fig. 416, vol. i. of the present work serves as well as those given by Pouchet, to exemplify it. The proligerous pellicle, due to the resolution into molecules of the primarily formified bacteriums and vibrios of

* CCCXII″. † CCCX″, pp. 122, 135.

‡ CCCXII″, p. 101: paraphrasing Pouchet:—'Si les œufs tombaient de l'atmosphère, comme le prétendent les panspermistes, il n'y aurait pas de raison au monde qui pût faire que, dans la même portion d'air, l'éprouvette en soit constamment remplie et la cuvette jamais. Celle-ci même, à cause de sa surface bien autrement étendue, devrait en récolter infiniment plus.'—CCCX″, p. 136.

§ CCCX″, Pl. II, figs. 1–5. and CCCXI″, Pl. I, fig. 1..

infusions, answers to the molecular contents of the ovisac. In both instances the molecules or granules aggregate into groups forming spheroids more opake than the rest (as in fig. 555, A): as the aggregation and coalescence advances the sphere becomes more opake, more definite: then a clear line marks its inclusion within a membrane, analogous to a 'zona pellucida,' and proclaims its individualization (as in ib. B).

Next appears a clear nucleus, answering to the germinal vesicle (as in ib. C). Fission of the nucleus is followed by that of the monad, which may thus multiply itself within the primary envelope (*Chlamydomonas*, CCXLIX, fig. 29), like the cleavage-formation of the germ-mass: ciliary organs are acquired in both instances, rotating the germ-mass in the mammalian ovum, and extricating the monad from its proligerous bed; whereupon it revolves or darts along, a free animalcule, in the subjacent liquor of the infusion.

In neither instance is there any support, from observation, of the derivation of germ-mass or of monad by evolution out of a pre-existing cell: in both instances have the processes of epigenesis or building up *ab initio* been repeatedly seen and traced.*

In the case of the ciliate infusory the following are the primary or preliminary steps in the formation of the proligerous pellicle, or 'Burdach's mucous layer.' In the clear filtered infusion a slightly opalescent appearance precedes the formation of the thin superficial film. This consists of molecules of various sizes, the most minute testing the highest powers of the microscope. These molecules I attribute to the act of formifaction, which in reference to organic matter in solution corresponds with the crystalline aggregation of mineral matter in solution. Solution of organic matter, such as clear serum from a blister, inclosed in 'goldbeater's' skin or other close membrane, and inserted beneath the integument of a living Mammal—even distilled water which so placed obtains the elements of formifaction by endosmosis—shows its results in the form of granules, white blood-cells, pus-globules, &c. These experiments need repetition and modification mainly in reference to the objection that such 'leucocytes' might have wriggled their way, like *Amœbæ*, from without, through the close texture of the enclosing bag.† In the proligerous pellicle the larger molecules unite end to end, forming bacteriums, or less regularly into masses composing *Torulæ*: these send out parts which become jointed tubes, and may terminate in rows of sporules (*Penicil-*

* CCCX″, pp. 352–388. CCCXI″, pp. 133–253. CCCXII″, pp. 121–129. CCCXIII″, p. 1046. CCCXIV″, p. 974. CCCXV″, p. 467: Mantegazza spent sixteen consecutive hours in observing this genesis.

† CCXVII″.

lium) or capsules of such (*Aspergillus*) The bacteriums may, by further union and confluence, form vibrios. There is much activity, allied in character to the Brunonian movements;* which, after a time, ceases, and the bacteriums, vibrios, &c., are decomposed to constitute the secondary series of molecules in and from which the development of the higher ciliate Infusory takes place. The formation of the proligerous pellicle or 'secondary histolytic mass of molecules'† by the primary developments and resolutions of the organic material, is analogous to the formation of the germ-mass, in ovo, by the successive spontaneous fissions, assimilations, and ultimate coalescence of the progeny of the original germinal cell.

To meet the inevitable question of 'Whence the first organic matter'? the Nomogenist is reduced to enumerate the existing elements into which the simplest living jelly (*Protogenes* of Hæckel) or sarcode (*Amœba*) is resolvable, and to contrast the degree of probability of such elements combining, under unknown conditions, as the first step in the resolution of other forces into vital force, with the degree of probability remaining, after the observations above recorded, of the interposition of a miraculous power associating those elements into living germs, or forms with powers of propagating their kind to all time, as the sole condition of their ubiquitous manifestation, in the absence of any secondary law thereto ordained.

In this, the last general summary of work which I am likely to find time to complete, the expression of belief on one or two points where proof is wanting may be condoned. The chance of its being a help, or encouragement to any younger, more vigorous mind, bent upon grappling with such problems, outweighs any anticipation of trouble consequent upon the avowal.

It seems to me, then, more consistent with the present phase of dynamical science and the observed gradations of living things, to suppose that sarcode or the 'protogenal' jelly-speck should be formable through concurrence of conditions favoring such combination of their elements and involving a change of force productive of their contractions and extensions, molecular attractions and repulsions—and that sarcode has so become, from the period when its irrelative repetitions resulted in the vast indefinite masses of 'eozoon,' exemplifying the earliest process of 'formifaction' or organic crystallization—than that all existing sarcodes or '*protogenes*' are the result of genetic descent from a germ or cell due to a primary act of miraculous interposition.

Some, accepting the latter alternative, teach that, while

* ccxviii", p. 470, in all organic molecules, living or dead.
† cccxxxv", p. 10.

generations of the first-created sarcode have descended to us unchanged from the period of the Laurentian limestone, other sarcodal offspring have developed and improved, or have been selected, into all higher forms of living beings. I prefer, however, while indulging in such speculations, to consider the various daily nomogeneously developed forms of protozoal or protistal jellies, sarcodes and single-celled organisms, to have been as many roots from which the higher grades have ramified, than that the origin of the whole organic creation is to be referred, as the Egyptian priests did that of the universe, to a single egg.

Amber or steel when magnetized seem to exercise 'selection;' they do not attract all substances alike. To the suitable ones at due distance they tend to move; but, through density of constitution, cannot outstretch thereto; so they draw the 'attracted' substance to themselves. If the amber be not rubbed, or the steel bar otherwise magnetized, they are 'dead' to such power. The movement of a free body to a magnet has always excited interest, often wonder, from its analogy to the self-motion so common and apparently peculiar to 'life.'

A speck of protogenal jelly or of sarcode, if alive shows analogous relations to certain substances; but the soft yielding tissue allows the part next the attractive matter to move thereto, and then by retraction to draw such matter into the sarcodal mass, which overspreads, dissolves, and assimilates it. We say that the *Protogenes* or *Amœba* has extended a 'pseudopod,' has seized its prey, has drawn it in, swallowed, and digested it. No 'organs,' however, are recognizable; neither muscle, mouth, nor stomach.

If the portion of iron attracted by the magnet became blended with the substance of its attractor, the analogy thereto of the act of the amæba would be, perhaps, closer, more just, than that other analogy which is expressed by terms borrowed from the procedure of higher organisms.

From certain knowledge of the homogeneous, by some termed 'unorganized,' texture of *Protogenes* and *Amœba*, we cannot predicate of their having sensation or exercising volition. Given 'life' and suitable organic substance at due distances, the act of making contact seems as inevitable, as independent of any volition of the amæba, as in the case of amber or steel, given 'magnetization' and attractable substances at due distance.

The term 'living,' in the one case, is correlative with the term 'magnetic' in the other. Devitalize the sarcode, un-

magnetize the steel, and both cease to manifest their respective vital or magnetic phenomena. In that respect both are 'defunct.' Only the steel resists much longer the surrounding decomposing agencies.

A man perceives a ripe fruit: if he can and will, he stretches out his hand, plucks, brings to his mouth, masticates, swallows, and digests it.

The question then arises whether the difference between such series of actions in the man and the attractive and assimilative movements of the amæba, be less or greater, than the difference between these acts of the amæba and the attracting and retaining acts of the magnet.

More may be said on both questions than I have here space for; but when all is said, the question, I think, may be put with some confidence as to the quality of the ultimate reply and the affinity to truth, and liberty to accept it, in the equal respondent, viz: whether the amæbal phenomena are so much more different, or so essentially different, from the magnetic phenomena than they are from the mammalian phenomena, as to necessitate the invocation of a special miracle for their manifestation.

Magnetic phenomena are sufficiently wonderful, exemplifying, as they do, one of those subtle, interchangeable, may we not say 'immaterial,' modes of force which endows the metal with the power of attracting, selecting, and making to move, a substance extraneous to itself. It is analogically conceivable that the same CAUSE which has endowed His world with power convertible into magnetic, electric, thermotic and other forms or modes of force, has also added the conditions of conversion into the vital mode.

Nerve-force we know to be convertible into electric energy, and reciprocally: and from the electric force, so induced, magnetic and other modes have been derived (vol. i, p. 357). The direction, then, in which may be anticipated the replies to the ultimate question, will be toward an admission of the originating and vitalizing of the primary jelly-speck or sarcode granule, by the operation of a change of force forming part of the constitution of Kosmos; not contrary to its ordained laws, in the sense in which 'miracle' or the 'interposition of special creative act,' is rightly understood.

But from protozoa,* or protista, to plants and animals, the gradation is closer than from magnetized iron to vitalized sarcode. From reflex acts of the nervous system, animals rise to sentient and volitional ones.

* This is the better as well as older term: ζῶον being understood as 'life' generically, and before development has differentiated its manifestations into unambiguous 'vegetal' and 'animal' modes.

And with that ascent are associated brain-centers, progressively increasing in size and complexity. Arrest the development of the human brain at the point it has reached in the 'Aztec,' and the faculty of generalizing and giving expression to such generalizations is wanting. The Aztecs can articulate words, and apply the right noun to the thing, as e. g. 'bread,' 'chair;' but they cannot combine ideas into propositions, and say 'give me bread,' 'set me the chair.'

For such advance in intellectual acts more brain is essential. Compared with the normal state of brains in the brutes best endowed, so much more cerebral substance is required, and in such position, as to make the great and sudden rise, in the lowest grades of man, which is referred to in vol. iii, p. 144.

Thought relates to the 'brain' of man, as does electricity to the nervous 'battery' of the torpedo: both are forms of force, and the results of action of their respective organs.

Each sensation affects a cerebral fiber, and in so affecting it, gives it the faculty of repeating the action, wherein memory consists, and sensation in a dream.

A dog at the sight of a rabbit receives a sensation which induces a volition, and he barks with the excitement of the chase. He sleeps, and by suppressed barking and agitation of limbs reveals the fact that he dreams. Shall we obtain any further insight into the nature of the act or acts resulting in this sensation, memory, dreamy imagination, by saying that the perception of the rabbit reaches the 'soul' of the dog by the affection of its cerebral fibers? Is the 'soul' of the dog other than the personified sum of his psychological manifestations?

The 'sight' of the dog, is its faculty of vision, the 'soul' of a dog is its power of knowing what it sees and determining accordingly: it may approach the object with every manifestation of sentiments of gladness and submissive affection: it may rush upon it with every sign of rage: it may pursue it with every mark of excited ardor.

And these mental activities can only go on for a time: the waste thereby occasioned of fiber and of power calls for renovation, and this for repose, of the mental organ. In sleep the eyes close and sight goes; what then happens to the brain-fibers we cannot see nor tell: but the sum of action called 'soul' ceases. Deep sleep is utter unconsciousness to dog and man. The initial steps and partial resumptions of brain-action are 'dreams'; the awakening one issuing, often suddenly, in the full blaze of consciousness.

I am most averse to travel beyond my proper province; but a general physiological conclusion from the phenomena of the nervous system inevitably brings on collision with a dogmatic

affirmation or definition of the cause of the highest class of those phenomena instilled as an article of religious faith into fellow-christians, and on which is based their mode of thought, affecting dearest hopes and highest aspirations. It must be repugnant to any good man's feelings to say aught that may unsettle such mode of thought, though he knows that what he has to impart lends truer and better support to both the faith and the hope.

If the hypothesis that an abstract entity produces psychological phenomena by playing upon the brain as a musician upon his instrument, producing bad music when the fibers or cords are out of tune, be rejected, and these phenomena be held to be the result of cerebral actions, an objection is made that the latter view is 'materialistic,' and adverse to the notion of an independent, indivisible, 'immaterial,' mental principle, or 'soul.'

What 'materialistic' means, in the mind of the objector, I nowhere find intelligibly laid down; but it is generally felt to be something objectionable, 'inconsistent with, or shaking the foundations of an article of faith,' as Stillingfleet would have said.

To this I repeat Locke's answer, that my faith in a future life and the resurrection of the dead, rests on the grounds of their being parts of a divine revelation.

If I mistake not, present knowledge of the way in which we derive ideas of an outer world, helps to a more intelligible conception of 'matter,' 'substance,' 'immateriality,' &c., than could be framed by patristic and mediæval theology. To make intelligible my own ideas in this subject, which the anticipated imputation draws from me, I would put a case and ask a question.

When Saul at Endor "perceived that it was Samuel,"* lines of force, as 'luminous undulations,' struck upon his retina. *Qu.* Were the centers whence they diverged to produce the idea of the dead Prophet 'material' or 'immaterial'?

Other lines of force, undulated in another manner, from centers producing the ideas of the dead man's speech:—"Why hast thou disquieted me, to bring me up?"† *Qu.* Were the centers radiating these acoustic lines of force material or spiritual?

Substitute the living for the dead Prophet, and it will be said that the points whence the rays of light converged to produce his image in the beholder are 'material,' because 'tangible;' in the case of the 'spirit of Samuel,' not. Had Saul stretched forth his hand to grasp the vision, it would have met

* 1 Sam. xxviii, 14. † 1 Sam. xxviii, 15.

no resistance. Let us, then, analyze the sensations from tangible lines of force. I stretch forth the sum of forces called 'hand,' and exercise part of them in a way and direction called 'pressure,' deriving the sense or idea of such act by my lines of force being opposed by other lines of force. To the extent to which my forces overcome the opposing forces, I have an idea of a something giving way; when my lines of force are overcome by the opposite lines of force, I have the idea of a hard or resisting surface. But all that I know, after ultimate analysis, is the meeting of opposite forces; of the centers respectively radiating such force I know nothing; and if I did or could know anything, I cannot conceive that I should get a clearer idea of 'touch,' than as a relation of certain lines of force acting from centers, which may as well be 'immaterial' as 'material,' for any intelligible notion I can frame of those verbal sounds.

If a blade of metal could move itself to and fro, in striving to cleave the space between excited electro-magnetic poles, and could tell us its sensations, they would be those of sawing its way through a substance like cheese; but there is no visible impediment: nor, were luminous undulations to vibrate from the hindrance, as from the plane of force resisting the pressing finger, would the hindrance be less 'immaterial.' Similarly, if lines of thought-force were visible, the 'ghost' would not on that account be more 'material.'

The ideas excited by the act of pressure are those of the 'exertion of force' and the 'resistance of force;' if these ideas be analyzed they include those of the direction of force in lines from centers or points. Further than this, my mind, or thinking faculty, cannot go; i. e., can have no clear ideas: I cannot feel that I know more about the matter by calling the 'centers of force,' 'material atoms' or 'immaterial' points, and am resigned to rest at a point beyond which Faraday* did not see his way.

Having evidence of the opposing force acting in lines from centers distinct from and outside of those volitional centers called 'ego,' the sensation is sufficient for my belief that it is due to the reaction of lines of force from outside-centers upon lines of force put into action from inside-centers. But I have no ground for calling the one 'material' and the other 'immaterial,' or either, or both. The same result has followed my attempts to analyze all sensations and volitions, i. e., I know of nothing outside myself of which I can have any clearer knowledge by calling it 'material,' than I have of that which

* cccxxxvii", p. 119.

originates force from within myself, by calling it an 'immaterial' entity, mental principle, or soul.

But, so it is; in the endeavor to clearly comprehend and explain the functions of the combination of forces called 'brain,' the physiologist is hindered and troubled by the views of the nature of those cerebral forces which the needs of dogmatic theology have imposed on mankind.

How long physiologists would have entertained the notion of a 'life,' or 'vital principle,' as a distinct entity, if freed from this baneful influence, may be questioned; but it can be truly affirmed that physiology has now established, and does accept, the truth of that statement of Locke,—'the life, whether of a material or immaterial substance, is not the substance itself, but an affection of it.'* Religion, pure and undefiled, can best answer, how far it is righteous or just to charge a neighbor with being unsound in his principles, who holds the term 'life' to be a sound expressing the sum of living phenomena; and who maintains these phenomena to be modes of force into which other forms of force have passed, from potential to active states, and reciprocally, through the agency of these sums or combinations of forces impressing the mind with the ideas signified by the terms 'monad,' 'moss,' 'plant,' or 'animal.'

If the physiologist rejects the theological sense of the term 'life,' without giving cause for the charge of unsoundness in religious principles, does he lay himself more open to the charge, by rejecting also the theologian's meaning of the term 'spirit,' of the term 'soul,' of the term 'mind,' and, we might add, of

* cccxxxvi", vol. i, p. 761. As the authority of a Physiologist and late President of the Royal Society may be cited for ascribing such vital phenomena to an invisible 'mental principle,'(a) I unwillingly refer to the remark by which Sir B. Brodie meets the obvious objection of the divisibility, without destruction, of acrite organisms:—'It is true that one of our most celebrated modern physiologists, from observing the multiplication of polypi by the mere division of the animal, has come to the conclusion that the mental principle, which to our conceptions presents itself as being so preëminently, above all other things in nature, one and indivisible, is nevertheless itself divisible, not less than the corporeal fabric with which it is appreciated.' (p. 115.) The reader, eager for new light and guidance toward truth, naturally here expects the facts and arguments exposing the weakness or fallacy of the inference deduced from the polyp-phenomena. The sole remark is a charge of that kind called '*argumentum ad hominem*.' 'But it is to be observed,' (proceeds Sir B. B.) 'that, great as is the authority of Müller generally, in questions of physiology, in the present instance he is not quite an unprejudiced witness, inclined as he is to the pantheistic theory,' &c. (p. 116) Now, the charge is untrue; and, were it otherwise, affects not the point in question. Johannes Müller was of the school of inductive physiologists, opposed to Oken and others of the school of Schelling. He would not accept even the 'vertebral theory of the skull,' or 'general homologies;' but adhered to the party of Cuvier. He lived and died a sincere member of the Roman Catholic Church. Brodie's notion of a 'mental principle' seems to be a combination of 'vital principle' and 'soul,' *πνεῦμα* and *ψυχή*.

(a) Brodie's Sir B., 'Psychological Enquiries,' 12mo. 1854, pp. 103, 115, 167.

'sin,' or 'death'? That is to say, arguments based upon scriptural expressions of thought-force may be drawn from the like personifications of the aberrations and cessation of such force. Both poets and painters have, in each case, endeavored to realize and give shape to the abstractions.

When doubting Thomas obeyed the Lord's command, his fingers met resistance below what seemed to him the surface of the side, and, entering the wound, were opposed by a 'force' exceeding the 'force' they exercised.* The resulting idea was that the 'matter' of our Lord was there, but wanting where the spear had penetrated; the fact was the opposition of a force by a force, and the sensation of that opposition. We know of nothing more 'material' than the 'centers of force.' Our ideas of things without as within the 'ego,' are the action and reaction of forces, as 'material' or 'immaterial,' as the ideas themselves.

In this view is avoided the alternative of 'idealism' with denial of an external world, or that of the personifying the sum of mental phenomena as an 'immaterial indestructible soul,' contradistinguished from other sums of forces which are as arbitrarily styled 'destructible matter.' Sleep, stimulants, drugs, disease, concur by their effects in testifying that the kinds and degrees of mental manifestations are the result of corresponding affections and changes of structure of the brain.

How the brain works in producing thought or soul, is as much a mystery in man as brutes—is as little known as the way in which ganglions and nerves produce the reflex phenomena simulating sensation and volition.

But it is a gain to be delivered from the necessity of speculating where the 'soul' wanders when thought and self-consciousness are suspended; or how it is to be disposed of until the 'resurrection of the body,' glorified or otherwise; of which reintegrated sum of forces 'soul' will then, as now, be a parcel. If the physiologist and pathologist had done no more than demonstrate 'the universal law of our being,'† which cuts away the foundations of 'purgatory,' or other limbo, from the feet of those who trade thereon,‡ which makes 'judgment' follow death, without consciousness of a moment's interval,§ they would deserve the gratitude of the Christian world.

* CCCXXXVI″, vol. i, p. 656. The whole of Locke's 'Second Reply' to Bishop Stillingfleet may be read, with profit, in relation to the undesigned testimony borne by physiology to the clear good sense and affinity for truth in the philosopher's remarks on the relation of the dogma of 'immateriality,' 'indestructibility' and 'separability' of soul, to a Christian's faith in the resurrection of the dead, as resting on the grounds of divine revelation.

† CCCXXXVII″, p. 306.

‡ Not to mention the kindred baser brood of 'Spiritualists and Spirit-Rappers.'

§ For the importance of this conviction to 'practice,' see CCCXXXVI″, vol. i, p. 156, § 63. 'In comparing present and future.'

ART. VI.—*On some phenomena of Binocular Vision;* by JOSEPH LECONTE, Prof. Chem. and Geol. in University of South Carolina.

I. *Adjustments of the eye.*

Two kinds of ocular adjustment take place in every voluntary act of sight, viz: (1) a proper convergence of the optic axes so that they shall meet on the object of sight, and (2), an adjustment of each eye so that the diverging pencil of rays which enters the pupil shall be brought to perfect focus, and therefore produce a perfect image, on the retina. The first or *binocular adjustment* is necessary for *single vision:* the second or focal adjustment is necessary for *distinct vision.* The first is distinctly sensible for all distances within 100 yards and perhaps for much greater distances: the second is scarcely if at all sensible for distances beyond two yards.

To the two adjustments mentioned above may be added a third, viz: *contraction of the pupil.* The design of the contraction of the pupil is probably to increase the clearness of definition of the retinal image by cutting off the most divergent rays from very near objects, and thus to decrease the spherical aberration which is not entirely corrected in the eye by the form of the lens. The pupil, however, also contracts involuntarily under the stimulus of strong light, without regard to distance. This must be carefully distinguished from the adjustive contraction, which is to some extent at least voluntary.

These three adjustments of the eye, viz: binocular or axial adjustment, focal adjustment and contraction of the pupil are associated in every voluntary act of sight. They are accomplished by one act of volition. They are so intimately associated that they cannot be voluntarily separated. It is usually impossible to converge the optic axes on any point, without at the same time adjusting the lens and contracting the pupil in a manner suitable for perfect vision at that distance. Such inseparably associated movements are called consensual movements.

The binocular adjustment is well understood; there is no difference of opinion as to its necessity nor the means by which it is accomplished. But in regard to the focal adjustment there has been much difference of opinion among the best physiologists and physicists. Some have denied altogether the necessity, and therefore the existence of any adjustment: attributing the phenomena which are usually explained by this means, to mere transference of attention from near to distant objects, or vice

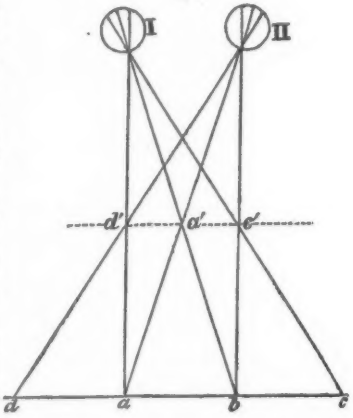
versa. The large majority of the best physicists and physiologists, however, have for a long time regarded focal adjustment as an optical necessity and therefore a fact; but the real nature of this adjustment and the means by which it is accomplished, has been a question in doubt. It has been attributed by some to the elongation of eye by the action of the recti muscles,—by others to the change in the convexity of the cornea—by others to the structure of the crystalline lens combined with contraction of the pupil—by others to the pulling forward of the crystalline lens by the ciliary muscle so as to elongate the chamber behind the lens, and by still others to the change of form of the lens by the action of the ciliary muscle. Recent very ingenious observations by Donders, Cramer and Helmholtz, upon the images of external objects made by reflection from the anterior surface of the crystalline lens, and the changes in form and size, which they undergo when the eye is adjusted for near objects, have definitely settled the question in favor of a *change in the curvature of the lens.* The mechanism by which this change is effected is not clearly known but it is probable that it is effected by the action of the ciliary muscle.

Before giving some experiments which bear upon the question of adjustment, I will state that my eyes are perfectly normal. In youth and early manhood the natural distance for distinct vision of small objects was eight inches, but with effort I could see perfectly distinctly at five inches. At the present time my natural distance for fine print is ten inches, though with effort I see distinctly at eight inches. Beyond this there is for me no limit of distinct vision. My eyes define the edge of the moon as perfectly as they do an object at the distance of ten inches. Moreover, by long practice I have acquired considerable and perhaps very unusual facility in making experiments on binocular vision and in analyzing my visual impressions. The following experiments which I have practised from boyhood, are interesting not only as a beautiful illustration of the laws of binocular vision, but I believe as throwing some light on the subject of adjustment and also upon the difficult subject of the *Horopter.*

If a plane surface checkered or otherwise figured *in regular pattern*, such as an oil floor-cloth, a tesselated pavement or a papered wall, be placed before the eyes at the distance of several feet, and the optic axes be then voluntarily converged (the eyes crossed) upon some point in space nearer than the surface, the figures will of course be all seen double. If now the convergence be steadily increased until two contiguous similar images, one belonging to the right eye and one to the left,

are made to coincide perfectly, and the eyes be then held steadily in this position for some time, the patterned surface will be distinctly seen in exquisite miniature, not at its proper distance but between the real object and the eye, at a distance depending upon the interval between the centers of the contiguous similar figures of the pattern. If the pattern be very regular the illusion is complete—we actually seem to be looking at a real object. In this experiment the position of the eyes is such that, of two contiguous similar figures, the right eye is directed toward the left figure and the left eye toward the right figure; and the image is seen at the crossing of the visual lines. Thus if one eye be directed toward *a* (fig. 1) and the other toward *b*, a perfect image of these two figures will be seen at *a'*. So also *b* and *c* will be united and seen at *c'* and *a* and *d* at *d'*, and so on for all the figures of the pattern. The dotted line *d' a' c'* will be the position of the image surface. The image thus obtained may be a little indistinct at first, but it gradually grows perfectly clear. As soon as the image is distinctly seen and the outlines of the figures well defined, it may be retained without any difficulty; for we seem to be looking at a real object and therefore retain the necessary convergence of the optic axes with ease. The eyes may now be turned in every direction, viewing this extensive image surface precisely as if it were a real surface.

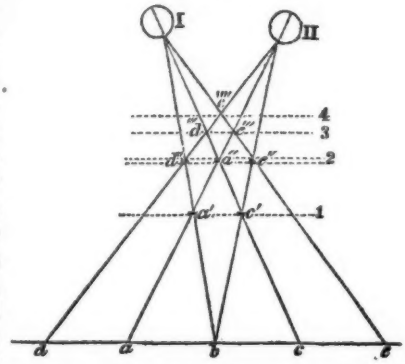
1.



If now while viewing the image in the last experiment we repeat upon it the same experiment, i. e., if by increasing the convergence of the optic axes we bring again the two contiguous figures in coincidence, a new image is formed between the last and the eye and is seen in still smaller miniature. In this case the position of the optic axes is such that the eyes crossing are directed not toward contiguous figures of the real object but to figures separated by an intervening one. Thus in the figure (fig. 2) *a* and *c* will be combined and seen at *a''*, *d* and at *d,''* and *b* and *e* at *e''*.

Upon this second image the same experiment may be repeated so as to make a third image still smaller and nearer the eye at d''' e''' and from the third even a fourth and still smaller image may be formed at e''''. The position of these successive planes are indicated by the dotted lines; but in this figure the position of the axes is only adapted to vision on plane No. 2. For the higher planes, the optic axes must converge still more. For the 4th plane I*e* and II*d* will represent the visual lines. Standing erect and looking down upon the regularly checkered carpet on the floor of my room, the figures of which are $4\frac{1}{2}$ inches from center to center, I can with the greatest ease bring out successively four distinct images one above the other, the nearest being but seven inches from my eyes, and the figures (which are two inches in diameter in the carpet) reduced to about $\frac{1}{4}$ inch in diameter. If while looking at the image on the 4th plane the convergence of the optic axes be suddenly relaxed the image drops and may be caught on No. 3. Again by relaxing the convergence it may be dropped and caught on successive planes until it falls to its natural position.

2.



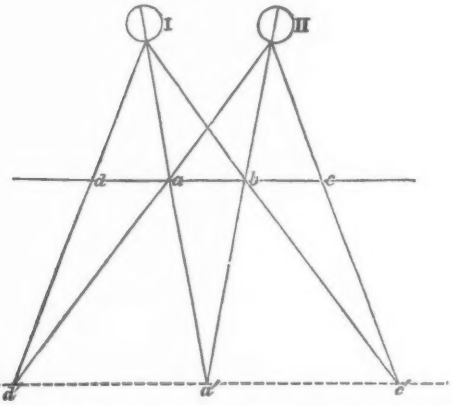
I have made similar experiments on a great variety of patterns of wall papering, oil-cloths, calicos, &c., with the same results. Of a regularly checked oil-cloth on my hall, the lozenge-shaped figures of which are 10·2 inches across, I make successively three perfectly distinct images, the nearest being but $4\frac{1}{2}$ inches from the eyes.

Those who are not accustomed to experiments of this kind can probably most easily succeed as follows. If we look on the floor, and place the finger between the eye and the floor the finger will of course be seen double. Now move the finger up or down until we find a place in which the two images of the finger will exactly fall on contiguous figures of the pattern: the finger now indicates the position of the first plane. Now look steadily at the finger instead of the floor, until the image of the floor rises to it and becomes distinct, then withdraw the finger. To get the second plane, look again at the floor and

raise the finger until its images fall upon figures separated from one another by an intervening figure and then look steadily at the finger. The other planes may be obtained in a similar manner. The position of the several planes may be also easily calculated; the data being the inter-ocular line, the distance of the object and the interval from center to center of the figures. Both by measurement and calculation I determined the planes in the case of the carpet to be 21·5, 13·05, 9·37 and 7·3 inches respectively. In the case of the oil-cloth they were 11·8, 6·54 and 4·5 inches.

If the distance between the centers of contiguous figures be less than the inter-ocular line, then still other images may be seen *beyond* the real object and very much *enlarged*. The position of the eyes and the place of the image in this case also

3.

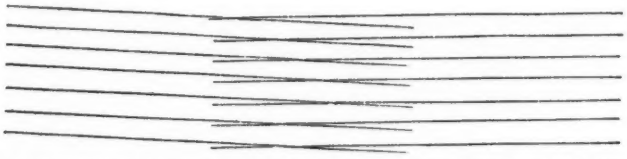


is easily explained. If *d a b c* (fig. 3) be the plane of the real object, and the eyes I and II be directed toward contiguous figures *a* and *b* but *not crossed*, then the image of *a* and *b* will combine and be seen at *a'* the intersection of the visual lines. So also *a* and *d* will be seen at *d'* and *b* and *c* at *c'*, and the dotted line will represent the position of the image plane. In order to make this image we must gaze *through and beyond* the pattern until we observe the double images come together and coincide, and then fix the eyes steadily. The enlarged image gradually becomes distinct.

This experiment is much more difficult than the preceding. The pattern should not be too small, otherwise the difficulty is very great. In former years I had often performed the experiment with perfect success, but the wall papering I had used

for this purpose, had been destroyed and I found difficulty in again obtaining a suitable pattern. I therefore constructed a pattern by ruling black lines on a large sheet of paper so as to make perfectly equal squares $1\frac{1}{2}$ inches wide. With this simple diagram my success in all the preceding experiments was really marvellous. The colored patterns before used form far more beautiful images, but for scientific purposes the ruled diagram is far preferable. With this diagram standing upright before me at the distance of sixteen inches, I got with great ease seven successive images on this side of the object and one beyond. All the images on this side were defined, with great ease and *perfect distinctness* although the nearest, both by measurement and by calculation, was but three inches from my eyes, i. e., far within the limits of my distinct vision. With great effort I could obtain others still nearer. The nearest I actually retained and measured, was but $1\frac{1}{4}$ inches from the root of the nose, but I afterward found that there was no limit except the root of the nose itself. Within three inches, however, the

4.



images were no longer perfect, not from any want of distinctness of the lines, but because the horizontal lines of the two images were no longer parallel but crossed one another as shown in the figure (fig. 4), and therefore could not be made to coalesce perfectly. The explanation of this will be given in its proper place. The still nearer images, as for instance those within $1\frac{1}{4}$ inches could not be retained; the strain on the interior recti muscles of the eye was too great.

The image beyond the object is much more difficult to obtain with clearness, especially if the object be near the eyes. At the distance of two feet from the object, I obtained the image very clearly and without much difficulty, but on approaching to within ten or twelve inches it was only by patient trial for some time that it could be brought out with perfect distinctness. When the object was twelve inches from the eyes the image by calculation was found to be about thirty inches distant. By turning the diagram so that the diagonals were horizontal and similar points therefore more than two inches apart, the image was seen at the distance of about six feet. It had

the exact appearance of a tesselated marble pavement made up of squares nine inches on a side.

In all these experiments the least irregularity in the pattern shows itself very conspicuously in the image, not by indistinctness of outline of the figures, but by apparent inequality in the plane of the image. Thus in the carpet it shows itself by an apparent wrinkle; in the lined diagram by some of the lines rising like black threads stretched above the general surface of the image. This phenomenon is a familiar one in stereoscopy, and is used for detecting the slighest difference in two apparently similar patterns; as for instance between a genuine and a forged bank bill.

I believe any one, and particularly any young person with good eyes, can with practice succeed in all the experiments detailed above. Several of my family have tried them with success. Yet in all cases it requires some practice to succeed well. I can, even yet, always detect some difficulty on first trial after an interval of a few days. But after several hours practice the illusion is so complete that it is almost impossible to dispel it. The image is so real that in attempting to recover the real object by relaxing the convergence of the optic axes, the doubling of the lines causes the eyes instinctively to return to their former position and thus to restore the image. I have sometimes been actually obliged to look away in order to recover the real object.

The experiments detailed above, have an important bearing on some points in the theory of vision. It is the universally accepted doctrine among physiologists that the axial and focal adjustments of the eye cannot be dissociated. Helmholtz speaking of the consensual movements of the eye says, "We cannot turn one eye up, and the other down; we cannot move both eyes at the same time outward; we are obliged to combine always a certain degree of accommodation of the eye to distance [focal adjustment], with a certain angle of convergence of the axes [axial adjustment]."* He proceeds, however, to give certain peculiar conditions under which the first two laws may be violated, but none in which the last is violated. For many years I regarded these experiments as confirming the ordinary doctrine. I had observed in my first experiments on the carpet that each successive plane became more and more indistinct. I accounted for this by supposing that both the optic axes and the lenses were adjusted for vision on the plane of the image, while the light diverged from the floor five feet distant. It seemed to me a crucial experiment proving the necessity of

* Helmholtz, Croonian lecture, Proc. Roy. Soc., April, 1864.

focal adjustment, and the inseparable association of it with axial adjustment. On re-commencing these experiments a few weeks ago, however, I was struck with the fact that the figures of the images were far more distinct than the real figures were when a small object was viewed in the position of the images. To test this point fairly I placed two bone buttons in similar positions and on similar spots on the pattern and then brought their images in coincidence. At first the united image was indistinct but gradually it became perfectly defined—every thread-hole as clear and distinct as it is possible to conceive. I succeeded, though with greater difficulty, in getting a perfectly distinct image of the buttons on all the planes. It was evident therefore that the indistinctness of the figures of the image on the higher planes, was not the result of the want of focal adjustment but of imperfection in the pattern. The subsequent experiments with the ruled diagram proved this beyond the possibility of doubt. The images in this case were obtained with much more ease and the lines were defined with the most perfect sharpness even when the image was brought nearly to the root of the nose.

In all cases, however, the image when first obtained was a little indistinct, and then gradually became clear. With unpracticed eyes this interval of indistinctness is considerable, but becomes shorter and shorter with practice, until it almost disappears. When the image once becomes clear, it remains so, but there is then a sense, while looking at the image, of gazing beyond it,—or rather perhaps, there is a difference between the image and a real object which we cannot account for, but which is not a difference of distinctness. There is evidently an unnatural condition of the eyes which produces strain and fatigue.

There is but one possible explanation of these phenomena, viz., that the *optic axes and the lenses are adjusted to entirely different distances.* The three adjustments of the eye, viz., the axial adjustment, the focal adjustment, and the contraction of the pupil, have been so associated through successive generations, and the association so confirmed and strengthened in each individual by constant practice from the earliest childhood, that a single act of volition accomplishes them all. Under ordinary circumstances they are so indissolubly associated that neither can be accomplished without the others. But the experiments described above prove that under certain circumstances *the first two,* at least, *may be completely dissociated.* In these experiments when the image is first obtained the optic axes, the lenses, and the pupil, are all consensually adjusted for vision at the distance of the image: and hence

the image must be indistinct, for the rays diverge from an entirely different distance. But gradually the lenses adjust themselves to the actual divergence, i. e., for rays diverging from the real object, while the optic axes remain adjusted for the distance of the image. The difficulty experienced in dissociating these two adjustments causes the interval of indistinctness. The perception of the difference between the image and a real object is the sense of this dissociation. Consensual movements have been, perhaps, brought about by the necessities of single and distinct vision; Helmholtz has shown* that other consensual movements may be dissociated when the necessities of single vision require it; these experiments show that the consensual adjustments of the eye may be dissociated when the necessities of *distinct vision* require it.

I was now anxious to determine what part was taken by the pupil. Is the contraction of the pupil more intimately associated with the axial or the focal adjustment? This question has been discussed by E. H. Weber, Cramer and Donders.† Weber believes it is directly associated with the axial adjustment, Cramer and Donders with the focal adjustment. To test this question, while I was obtaining the image and making it clear, an assistant standing behind and a little to one side observed my pupil reflected in a small mirror conveniently placed. After gazing intently at the real object until the pupil was steady, as soon as I converged the optic axes so as to obtain the image No. 1, the pupil was observed to contract decidedly, but as the image became clear *it again expanded to its original size.* Again at the moment of obtaining the 2d image the pupil contracted still more strongly, but as soon as the image became clear it again expanded, nearly, if not entirely to its original size. The same phenomena were observed for each of the images, only that in the nearest images when the convergence of the optic axes was extreme and the first contraction very great, the pupil did not return entirely to its original dimensions.

I then made similar experiments on the image beyond the real object. As before, I looked intently first on the real object at the distance of twelve inches until the pupil became steady. So soon as I gazed beyond the object the pupil of course expanded; but as soon as the image became clear *it again contracted to nearly its original size.* In this last experiment the pupil is apt to be unsteady. This might have been expected; for as we have already said, it is much more difficult to obtain this image clear or to retain it when obtained.

There is no doubt of the fact, therefore, that *the contraction*

* Proc. Roy. Soc., April, 1864.

† Donders, accommodation and refraction of the eye, Trans., p. 574.

of the pupil is most intimately associated with the focal adjustment.

I believe that this principle of dissociation of consensual adjustments explains perfectly certain phenomena of the stereoscope. It is well known that many persons experience difficulty in seeing stereoscopic pictures distinctly even when the two pictures are brought into perfect coincidence: and I believe all persons experience some fatigue to the eyes in looking at stereoscopic pictures for a considerable length of time. I have often felt both the difficulty and the fatigue, though to a much less degree than most persons. The explanation of this difficulty is as follows. We judge of distance, as is well known by the axial adjustment. If then the two pictures are so taken that, in order to bring them together, the visual lines must meet at a certain distance, say fifty yards, then the picture will be seen at that distance and of course very much enlarged. But in order to see the picture clearly, the rays must come to the eye as if they diverged from the same distance; for the eyes are adjusted for that distance. To fulfil this condition lenses are always used; but it is obvious that a given pair of lenses are suitable for one distance only. For all other distances, or degrees of optic convergence, there must be some degree of dissociation of the two adjustments, and this is both difficult and fatiguing to most persons.

I have found that observations upon the images of the ruled diagram are a most delicate means of determining both the rotations of the eye, and the position of the Horopter. I hope in my next communication to take up this most difficult subject.

(To be continued.)

ART. VII.—*On the Geology of Lower Louisiana and the Rock-salt Deposit of Petite Anse;* by EUGENE W. HILGARD, PH.D., of Oxford, Miss. (Abstract.)

THE discovery in 1862, of a deposit of rock-salt on the coast of Louisiana, was a fact so unexpected to geologists, that at any other time a detailed investigation of its geological relations would quickly have followed the first announcement. The pressing necessities of the blockaded section soon caused its exploitation on the large scale, though in a very irregular manner; for a considerable period, these mines supplied the whole of the southwest. In November, 1865, Prof. Richard Owen made a brief examination of the locality, the results of which he published in the Transactions of the St. Louis Academy. A year

later Dr. Charles A. Gössmann, under the auspices of the American Bureau of Mines, made an examination of the locality, mainly with a view to the exploitation of the deposit; his report, published by the Bureau, as well as the specimens which he courteously exhibited to me, confirm previous conjectures that the overlying strata were the equivalents of the formation I have described as the "Orange Sand" of Mississippi. I therefore gladly availed myself at the earliest possible moment, of the offer of the Smithsonian Institution to defray my expenses in making a detailed geological investigation of the region. The low stage of water prevailing at the time (December, 1867,) rendered it possible to observe to the best advantage the formations exhibited on the banks of the Mississippi; the examination of which, from Vicksburg to the Passes, was a needful preliminary step to the determination of the formations of the coast.

Having previously examined and described the sections exhibited at Vicksburg, Grand Gulf and Fort Adams,* I merely landed at some intermediate points to verify the conclusion previously reached, viz., that below Vicksburg, no marine formation crops out on the river banks, reports to the contrary notwithstanding; and that the profiles at Natchez, Rodney and other points are essentially similar to that at Fort Adams, where we find the strata of the (fresh-water) "Grand Gulf group" in a position nearly or quite horizontal; overlaid, first by the materials of the "Orange Sand," which in its turn is capped by the stratum of the "Loess" or Bluff formation, covered by a thin deposit of "Yellow Loam."

Facing southward from the "Blockhouse hill" at Fort Adams, we observe a wilderness of the characteristic sharp ridges of the Loess region, often fore-shortened into veritable peaks, elevated between 300 and 400 feet above the river. In this region, the Grand Gulf strata have been traced southward by Dr. George Little, the present State geologist of Mississippi, as far as the head waters of Thompson's Creek, northwest of Clinton, La.

The Orange Sand proper is visible, near the river, as far south as Jackson, La., but farther inland extends to a lower latitude. As for the Loess, it appears in full force and characteristically developed for some distance south of Fort Adams. But (according to Dr. Little's observations) these features become gradually modified as we advance southward. The Loess deposit thins out, its materials become poorer in lime and fossils, and assume more and more the character of a common fine grained "hardpan;" the transition being by insensible de-

* See Report on the Geology and Agriculture of Miss., 1860.

grees, while the two extremes are very obviously distinct. At the same time, the clayey substrata which, farther above appear only in patches (as at Nevitt's bluff, two miles above Natchez, as well as at the latter place itself) are seen more frequently and continuously, until, at Port Hudson, they become predominant.

The exposure at Port Hudson, previously examined in part by Bartram, Carpenter and Lyell, is about three miles in extent, from the mouth of Sandy Creek above the town, to Fontania Landing, 1½ miles below. Its lower half is washed, and continually encroached upon, by the river; its upper portion is now inland of an extensive sandbar. The strata are disposed horizontally or basin fashion, and vary a good deal both in thickness and materials, as shown in the subjoined profiles, situated about a mile apart; the correspondence of strata is ascertained by actual tracing of the stratification lines.

Near Sawmill, Port Hudson.	No.	*Midway betw'n Port Hudson & Fontania.*
Yellow surface Loam, 4—6 ft.	6	Yellow Loam, sandy below; 8—10 ft.
Yellow Hardpan, 25 ft.	5	White and yellow hardpan, 18 ft. Orange and yellow sand, sometimes ferruginous sandstone, irregularly stratified, 8—15 ft.
Heavy greenish Clay, 7 ft.	4	Heavy, greenish or bluish Clay, 7 ft.
Gravel, sand and clay in irregular bands, like river alluvium; with pebbles, driftwood, leaves, and Mastodon bones, 6 ft.	3	White indurate silt or hardpan, 18 ft.
Heavy, greenish or bluish, massy clay, similar to No. 4; 25 ft. visible.	2	Heavy green clay, with porous calcareous concretions above, ferruginous ones below; some sticks and impressions of leaves, 30 ft.
	1	Brown muck, } with cypress stumps, 3—4ft. White or blue clay, }

At the stage of extreme low water prevailing at the time, the stump-stratum No. 1 was visible to the thickness of 10 ft. at its highest point; showing several generations of stumps above one another, also the remnants of many successive falls of leaves and overflows. The wood is in a good state of preservation; no prostrate trunks to be seen at present.

The main clay deposit, No. 2, varies but little in general character; although very solid, its tendency to cleave into prismatic forms renders it very liable to "cave" into the river. The upper portion of the stratum, especially near its southern end, contains strings of calcareous nodules, on stratification lines eight to twelve inches apart. No fossils save rare impressions of leaves.

No. 3 is exceedingly variable. At the northern end of the outcrop, it is a narrow band of swamp deposit; at the first of the profiles given, it bears the character of a sandbar; lower down, it returns to that of a swamp deposit; still below, it is represented by a fine white silt, without a trace of vegetable remains. Lower down again, a lignitic layer appears at its base, with leaves and fruit of living species of lowland trees; while near Fontania, it is again a sandbar, with an abundance of prostrate trunks of driftwood, coarse sand and pebbles.

The green clay stratum No. 4 varies little, either in thickness or composition, and like the stump-stratum No. 1, forms a convenient level of reference.

The hardpan stratum No. 5 I conceive to be the more immediate representative of the Loess proper, with which it is connected by gradual transition, though at times greatly resembling some of the materials of the Orange Sand. It is void of fossils.

The present profile differs in many respects from those given by previous observers, which lay some distance farther west, where the river now flows. The strata are accordingly as variable in an east and west, as in a north and south direction, and with the exception of Nos. 1 and 2, are such as are now shown in ditches cut into the modern river-bottom deposits.

The stump-stratum No. 1, however, as appears from numerous data collected by myself or contained in Humphreys and Abbot's Report on the Mississippi river, exists at about the same level (i. e., near that of tide-water) not only over all the so-called Delta-plain of the Mississippi, but also higher up, perhaps as far as Memphis, and all along the gulf coast, at least from Mobile on the east to the Sabine river. Wherever circumstances allow, the overlying clay stratum No. 2, is also observed. These facts indicate the wide spread prevalence, during the epoch succeeding the drift, of quiet, shallow fresh-water lagoons and swamps of slightly varying elevation; through which the continental waters may for some time have found an outlet without a definite channel representing the Mississippi of to-day. The Port Hudson profile appears to be typical, its features being reproduced wherever denudation has not removed these deposits down to the level of the stump-stratum, as is mostly the case.

The Five Islands.

The chain of five islands rising partly from the sea, partly from the coast marsh, between the mouth of the Atchafalaya and Vermilion river, have been described by Mr. Thomassy,*

* Géologie pratique de la Louisiane; New Orleans, 1860.

who attributes their origin to "hydrothermal" or "volcanic" action. His descriptions are sufficiently faithful to show the general resemblance of their geological structure; so that after visiting the three middle members of the chain, viz: Côte Blanche, Weeks's Island, and Petite Anse, I have thought it superfluous to extend my examination to the two extreme ones, viz: Belle Isle, the promontory west of Atchafalaya Bay, and Miller's Island (or "Orange Grove,") overlooking the plains of the Vermilion. These elevations lie nearly in a straight line bearing N.W. by W. from Belle Isle.

Côte Blanche.

The next in order, affords on its sea-face a fine exposure of the lower members of the Port Hudson profile. At tide-level, we have the blue clay with cypress stumps, the tops of which are often surrounded by alternate layers of clay, muck and sometimes lignite. The overlying strata consist, partly of blue clay similar to No. 2 at Port Hudson, partly of various colored loams alternating with the former; and exhibiting the same calcareous or ferrugino-calcareous concretions along the stratification lines. At a few points, these calcareous concretions resolve themselves into distinct fossils, representing the fresh-water genera *Paludina*, *Melania*, *Unio*, *Anodonta* and *Cyclas*, in an indifferent state of preservation. The entire visible profile is about 50 feet high; the highest point of the island rises as high as 180 feet, but in its interior no exposures exist, so that the higher members of the series are not verifiable.

Weeks's Island.

This island, lying 6 miles N.W. by W. from Côte Blanche, has an area slightly greater, viz., 2,300 acres; it is nearly circular; maximum elevation 160 feet above tide water. Unlike Côte Blanche, it is traversed by deep ravines which exhibit the geological structure. In the central and highest portion, these gullies are bordered by steep slopes composed of the most characteristic materials of the Orange Sand group. On the exterior slopes, however, we find in a position inclined away from the center of the island, the lower strata of the Port Hudson profile—green or blue clay with calcareous concretions, and imperfect fresh-water shells. The blue clay stratum with cypress stumps is met with in ditching, and is also known to exist in the beds of the neighboring bayous, as well as in the surrounding marsh.

Petite Anse, or Avery's Island.

Petite Anse lies about 12 miles N.W. by W. of Weeks's Island, and in its general structure much resembles the latter, to which it is slightly inferior in size, and about equal in elevation; its highest point, "Prospect Hill," on the north side, being 160 feet above tide-level.

An elevated ridge connects Prospect Hill with another high point near the southern slope of the island; and near the west end a ridge, on which Judge Avery's house stands, falls off steeply toward the Bayou Petite Anse. These three points inclose the valley in which the salt deposit has been found and which opens southeastward into the marsh.

The topography of the island, as well as the history of the mine, have been ably given by Dr. Chas. A. Gössmann of Syracuse, in a report of the American Bureau of Mines.* Up to the time of his visit, all the pits and shafts had been sunk through detrital strata, washed down from the adjoining hills, and frequently inclosing the vestiges of both animal and human visits to the spot. Mastodon, buffalo and other bones; Indian hatchets, arrow-heads and rush baskets, but above all an astonishing quantity of pottery fragments, have been extracted from the pits. The pots doubtless subserved the purpose of salt-boiling; human handiwork has, however, been found so close to the surface of the salt, as to render it likely that its existence in mass was once known, before the time when, in 1862, Mr. D. H. Avery struck the salt itself at the bottom of a salt water well.

The surface of the salt undulates considerably, so that borings commenced at different levels have repeatedly struck salt at nearly the same relative depth, the absolute level of the rock-salt surface varying from 32 ft. below to 1¾ ft. above tide level. The salt stratum has itself been penetrated to the depth of 38 feet, without any perceptible variation in quality; its "floor" being as yet unknown. Dr. Gössmann's observations and specimens proved to his and my satisfaction, the existence of the Orange Sand on the Island; but its relation to the rock-salt, and the age of the latter, remained undetermined.

Since then, another shaft has been sunk by Mr. Chouteau of St. Louis, with the assistance of Mr. Dudley Avery, to whom I am indebted for a record of the strata penetrated. This shaft was located at a higher level than any previously sunk, on a hillside where, not far off, the Orange Sand crops out *in situ*. After passing through these strata, the rock-salt was struck again, at a level several feet higher than on any former occasion.

* On the rock-salt deposit of Petite Anse, New York, 1867.

There can therefore be no doubt that the salt deposit is older than the Orange Sand, which here as at Weeks's Island, forms the nucleus of the mass on whose outer slopes, as well as its higher points, the strata of the Port Hudson profile reappear characteristically; with calcareous nodules, fresh water shells and aquatic plants identical with living species. Not only is the reference level of the cypress stump stratum the same as elsewhere, but the green clay band, No. 4 of the Port Hudson profile, is also there.

The stratigraphical disposition of these deposits is quite remarkable. They conform not to the *strata*, but measurably to the *outline* of the Orange Sand nucleus, roughly following its slopes and curvatures. At first sight therefore it seems as though a local upheaval had taken place, and hence arose, probably, the reports attributing a volcanic origin to these elevations, whose isolated position in the level coast region would naturally give rise to speculation as to their mode of formation. Indeed the extent to which these strata are sometimes seen to dip, rather staggers the observer; but the upheaval hypothesis does not explain the facts, unless we are content to assume a separate effort of the sort for every hillock on the islands.

There can be no doubt that subsidence subsequent to deposition has been the cause of the extravagant dips observed sometimes. Where the Port Hudson series is more immediately superimposed upon the Orange Sand nucleus, the dips are moderate, and such as may well be assumed as resulting from deposition on inclined surfaces. But when we see an apparently undisturbed clay-stratum moving down hill like a glacier, so as to overflow a deposit of loose stones, we need not go far to find the cause of extensive dislocation and subsidence.

Belle Isle and Miller's Island.

All the data I have been able to collect concerning the structure of these exterior islands, tend to confirm the probable supposition that, like the three interior ones, they consist of denuded nuclei of Orange Sand materials, upon which the Port Hudson series was afterwards deposited.

It seems likely that the same is true of a low ridge called Côte gêlée, in Lafayette parish, bearing N. or N.N.E. Thomassy places in the same category the Grand Côteau des Opelousas and the Avoyelles prairie.

Age of the Salt Deposit.

The Orange Sand strata so rarely approach the coast, that the deposits underlying them in the Coast region have scarcely

been observed with certainty. Even the older strata underlying the blue stump clay have been observed at a few points only, viz: by the Delta Survey in the bed of the Mississippi river at Bonnet Carré and Carrollton, near New Orleans; at the latter city itself, in the boring of wells; at Salt Point, on Bayou Salé; and on the coast of Mississippi Sound.

The strata penetrated in the borings at New Orleans are considered by Sir Chas. Lyell as Delta deposits. But according to my examination, they are almost throughout demonstrably of marine origin, and while the species they contain are mostly (not all) now known to be living on the Gulf coast, yet the prevalence of species is very different from that now observed near the mouths of the Mississippi. In this respect, the fauna of these strata shows a great analogy to those described as Pliocene by Tuomey and Holmes, occurring on the Carolina coast.

It is most probable that the rock-salt of Petite Anse will be found when pierced, to be imbedded in the equivalents of the deposits penetrated at New Orleans and Bayou Salé, and of corresponding, probably early quaternary age, anterior to the drift or its southern representative, the Orange Sand.

Origin and extent of the salt deposit.

The absence of layers of the usual impurities of rock-salt, especially of gypsum, has induced Dr. Gössmann to suppose that it is not the result of the evaporation of sea-water, but owes its formation to crystallization from the purer brine of salt springs.

Our knowledge of the facts is still too limited to render a discussion of this point very profitable. In a very deep lagoon, withdrawn from the influx of the tides after the brine had acquired a considerable degree of concentration, all the gypsum might be found in a single bed at the bottom; upon it a large mass of pure salt, as in the present case; while the salts of the mother-waters would naturally have been washed away from the top. Or there might have been a succession of lagoons communicating with each only during high tides, and acting in a manner analogous to the process now practiced in salt-making on the sea-shore. The gypsum would then all have been deposited in the outer lagoons, while the inner ones would have acted as brine-pits, where pure salt alone could crystallize. Crystals of gypsum have repeatedly been found in shallow wells on the coast, beneath the "stump clay."

Upon any of the foregoing suppositions, calling into play a variety of circumstances not likely to be all simultaneously fulfilled, it does not seem probable that the rock-salt mass is

very extensive horizontally, or that such masses should occur frequently in the coast region.

A mass of salt 144 acres in extent and 38 feet thick is, however, a handsome specimen, even if these dimensions should represent maxima. The great difficulty in mining it, heretofore, has been the influx of water through the gravelly strata overlying. But it has most probably been attacked, thus far, at its lowest surface level. Wherever elsewhere the Orange Sand formation prevails, it rests on a deeply denuded surface; and "hills within hills" are of very common occurrence. From the data thus far obtained it appears that the same is the case with the rock-salt mass, and that its surface roughly conforms to the hills and valleys now existing. Workings should be begun at higher levels; and it would not surprise me to learn that the auger had shown the mass to be accessible by level adits in lieu of shafts, on the hillsides. The interior of the solid mass once gained from a point secure from surface water, all difficulty would be at an end.

Geological History of the Lower Mississippi Valley.

It appears from the facts stated in the preceding pages, that after the termination of the epoch of that Eocene period, represented by the Vicksburg group of fossils, down to the Quaternary era, marine deposits ceased to be formed on the northern border of the basin now represented by the Gulf of Mexico.

I have acquired the certainty of the existence, over a large portion of northern Louisiana, of the "Grand Gulf" series of rocks. From specimens in the collection of the New Orleans Academy of Sciences, it appears that apart from the usual materials forming these beds in Mississippi, they assume in the Harrisonburg region the character of compact limestone, which in places is said to be fossiliferous, and would thus furnish the clue to the age of the Grand Gulf group, for which I have vainly sought in Mississippi. The problem is one of great interest, as it involves the question whether or not the Mexican gulf has, within comparatively modern times, been disconnected from the Atlantic ocean. The absence of the cauldron in which the Gulf Stream is concocted might have exerted climatic influences reaching beyond the American continent, and would explain many discrepancies between ancient and modern faunas on the shores of the Atlantic.

It appears that similar limestones, almost assuming the character of black marble, occur in St. Landry parish, near Opelousas. Whether the southern outline of the formation passes thence toward the Calcasieu region, where petroleum has been found, or whether it trends northwestward into the par-

ishes of Sabine and Natchitoches, where limestone and sandstone ridges also exist, is a question still open. In the latter case, this outline would conform to the general shore lines of the great cretaceous and tertiary Mediterranean.

In Mississippi, the Grand Gulf series is mostly overlaid by the Orange Sand, deposited on a deeply eroded surface, and bearing itself the evidence of its formation by fresh water in a state of violent flow.* The southern outline of the main body of the Orange Sand runs southward of Opelousas, toward the mouth of the Sabine, whence, according to reliable information, a broad band of shingle extends toward Harrisonburg, Catahoula parish. This belt represents, probably, the most westerly bayou of the great Orange Sand Delta; while, as heretofore stated, the most easterly one extends from the neighborhood of Cairo along the western shore of the Tennessee river, down the valley of the Warrior toward the coast of Alabama. The middle and main pebble-stream evidently follows in general the course of the Mississippi river; but leaving it at the point where that river suffers its remarkable deflection eastward, we find the remnants of its ancient "bar" in the chain of the "Fire Islands," which lie directly across the shortest line by which the Mississippi could reach the Gulf, and no doubt have had their share in causing this deflection.

Both the size of the pebbles carried by this middle bayou, and their character proving transportation from high northern latitudes, show it to have been the main channel during the Orange Sand epoch. It is not surprising, therefore, that in the direction of its course the Orange Sand formation should extend farther south than anywhere else. The pebble-beds are now overlaid by fine sandy materials, proving a diminished velocity, owing, doubtless, to a general depression, but greater at the north than at the south.

While the lateral bayous descending through Louisiana and Alabama were closed at the end of the Orange Sand epoch, it is evident that the central channel continued open; inasmuch as the next succeeding deposit, viz: the Loess, lies in a trough-shaped depression of the Orange Sand materials, the line of contact being always conformable and devoid of any trace of atmospheric denudation. The perfect peroxydation of the materials of the Orange Sand would seem, nevertheless, to point to a certain period of exposure to atmospheric agencies, caused by a temporary diminution of the influx of northern waters, through the cessation of subsidence, perhaps.

During this epoch of quiet might have begun the formation of those extensive swamp and lagoon deposits, the lower mem-

* Am. Jour. Science, May, 1866; Miss. Rep. 1860, p. 26 and ff.

bers of the Port Hudson series, whose floor stratum, with its superimposed generations of cypress stumps, indicate a slow secular subsidence. The velocity of the latter seems gradually to have increased until the growth of old trees became impossible, and finally, in stratum No. 3 of the Port Hudson profile, we again meet the evidences of currents moving sand, pebbles, and drift-wood.

Then follows the Loess proper, a deposit utterly devoid, in Mississippi and Louisiana, of any evidences of fluviatile action —a uniform silt even in profiles of 80 feet, with scarcely a vestige of stratification, and none but terrestrial fossils.

The precise circumstances under which such a deposit could be formed, are perhaps a little obscure. There must have been such a depression of the whole country as to transform the immediate valley of the Mississippi, as far as Keokuk, as well as the valleys of the larger tributaries, into estuaries of the Gulf of Mexico, containing a mass of water too great to be sensibly affected by the variations now causing the annual overflows of those rivers (for otherwise the deposits must have shown lines of deposition), yet possessing a gentle flow above (since the materials of the bluff formation of Missouri and Indiana exhibit signs of fluviatile action); quite fresh in its upper portions (where fluviatile shells are found), but rendered unfit for the life of either a fresh or salt-water fauna by an admixture of sea-water, in its lower and almost stagnant portion, at tide level; and deriving its vestiges of animal life only from the "offscourings" of the adjoining unsubmerged lands.

Sir Chas. Lyell* inclines to consider the Loess as the product of "successive inundations of a great river," the absence of stratification from such deposits having, apparently, an analogue in the alluvial deposits of the Nile. But the case is far from being analogous; for the same phenomena are still observed in the modern deposits of the Nile, and are clearly attributable to the peculiarities of the hydrographic basin of that river; whereas, in the modern alluvium of the Mississippi, it is exceedingly difficult to find a uniform stratum two feet in thickness. The Nile mud is each year derived from the same rivers of Abyssinia, and equalized by intermixture and subsidence during at least 1,500 miles of its course. On the Mississippi, on the contrary, the deposits of different annual inundations are readily distinguished by the inhabitants for years afterward, according as the Illinois, the Missouri, Ohio, Arkansas or Red river happen to have furnished the main influx. The absence of any such differences from the Loess can only be explained on the assumption that the mass of

* Principles of Geology, 10th edition, p. 464.

water filling the channels was too great to be sensibly affected by such causes, the more so as the continental surface was sensibly diminished in consequence of a depression which, as far south as Fort Adams, cannot have been less than 400 feet, and on the coast not less than 200, but more probably the same as farther above.

The existence of the elevations on the Louisiana coast, above described, renders it necessary to assume that at the end of the period of depression—the "Champlain epoch"—the entire delta-plain (so-called) west of the Mississippi was covered by the deposits of the Orange Sand and Port Hudson series to an equal height; and that during the succeeding "Terrace epoch" of elevation, the veritable Mississippi—*our* Mississippi—swept away these deposits in excavating its present valley. At first it might sweep over or through the pebble ridge, but would finally turn to the direction of least resistance, leaving the "Five Islands" high and dry.

It would thus seem that, unlike other large rivers of the world which have from the outset added to the land by bringing down the materials to form their alluvial plain, the Mississippi has first formed by denudation the plain which it was subsequently to cover with its alluvial deposits to a comparatively inconsiderable depth. The western and southern limits of this denuding action would seem to be marked by the Grand Côteau des Opelousas, the Côte Gêlée and the Five Islands; and the materials swept away from this area doubtless contributed largely to form the foundations of the truly alluvial plain extending south and southeastward of lakes Maurepas, Pontchartrain and Borgne.

It is obvious how futile must be all attempts to estimate the age of the Mississippi river in absolute measure, by a comparison of the advance of its present delta into the Gulf, with the distance of its mouth from the divergence of bayous Plaquemine or Manchac. When the broad flood of the Terrace epoch contracted into the present Mississippi, that stream emptied into a sea rendered shallow by the deposition, within a comparatively short period, of a huge amount of material. Within such a sea its channel would be likely to change about, somewhat like those of the great rivers of China. *Now*, it is advancing into the deep water of the Gulf of Mexico, but at a very different rate, and by a very different process from that of simple alluvion. But the questions pertaining to this portion of the subject, together with the results of my observations in the Delta proper, I propose to discuss at a future time.

ART. VIII.—*Notes on the recent volcanic disturbances of Hawaii;* by Rev. TITUS COAN;* (from a letter to J. D. Dana, dated Sept. 1, 1868).

I HAVE recently returned from a tour of eighteen days in Puna and Kau, during which time I visited all the important points of interest, took measurements, and made careful observations. I take the liberty to give you a few facts and remarks from my notes.

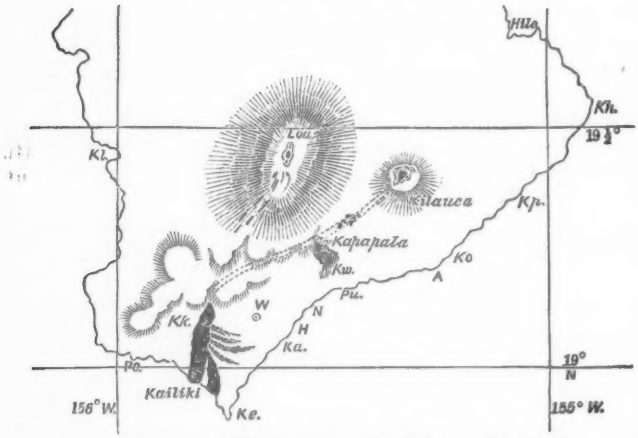
I left Hilo on the 4th of August and took the shore road through Puna. From Hilo to the east cape, Kapoho, the disturbances were not remarkable. Walls and rocks had been, more or less, shaken down, and fractures appeared, here and there, in the earth. From the east cape to the south point of the island, Kalae, the disturbance had been great. The whole coast line appears to have undergone a subsidence of unequal depth. At Keahialaka, about seven miles southwest of Kapoho, where there has been a small pool of brackish water, passed by a causeway of stones, the water now stands, at high tide, three to four feet deep, and spreads out among cocoa-nut groves where water was never seen before. At Kaimu, the sea has driven in a heavy beach of lava sand for two hundred feet into a beautiful young cocoa-nut grove, and spoiled a delightful lawn. At Kalapana, the sand beach has been forced into groves of Pandanus and cocoa-nut, where the trees now stand six to eight feet deep in sand, and the shore line has been pressed back the distance of 100 feet. The old stone church is nearly buried in sand, and the tide rises and falls within its walls. t formerly stood 200 feet from the water. The plain of Kala-

* Mr Coan also sends us the following by way of correction of our newspaper extracts in the July number of this Journal: "Article XIII of the Journal of Science for July has just come to hand. I regret that several important errors have been published and very widely circulated. But this could hardly be avoided in times of such terrible excitement, when exaggerated reports filled the air, and the honest eye and ear sometimes failed to report the exact truth. I am surprised to find myself reporting 2,500 to 2,600[1] head of cattle destroyed. Did I state that? If so, I stated just what was reported to me. The truth now appears to be, that on Kahuka, where 200 cattle were said to have been roasted in a holocaust, by a puff of Pele, only 35 perished, as I learn from the owner. And on the Kapapala ranch, one of the partners sets the number at 1,500, the other at 500, while some of the neighbors would reduce the loss to 200. But on a ranch of 5,000 cattle, spread over all the southeast and eastern flanks of Mauna Loa, it is impossible to tell the exact number destroyed by the landslide. The statements, also, that a vast lava stream went, under ground, into the sea at Punaluu, and that another broke out at Hiilea, were optical illusions of Mr. Lyman and many others. They spoke and wrote what, at a distance, appeared to them as facts, but which proved to be errors on careful inspection. So also the report, that the sea-waves rolled over the tops of cocoa-nut trees, sixty feet high, was an error by two-thirds.

[1] In my letter published in the Missionary Herald for July, you will see that I say 500 to 600 head of cattle, which is nearer the truth."

pana has sunk four to six feet, and many acres of once dry land are now covered with water three and four feet deep. Bathing coves, also, once having spaces of three, four and six feet between the water and the roof, are now full or nearly so. At Kealakomo, the salt works are all destroyed, and the spring of cold water among the rocks near the shore is sunk. At Apua, the most western village in Puna, all the houses, the crescent sand-beach, and the beautiful little canoe harbor are obliterated—the fishermen mourn. Water now stands where the village once stood. The same is true of Keauhou, the most eastern village of Kau. The subsidence there is seven feet by measurement upon cocoa-nut trees. This place has an ample and safe anchorage, and here was the station where large quantities of pulu were dried, pressed and shipped for market. The influx of the sea destroyed the buildings, but the proprietors have built again on higher rocks.

PART OF THE ISLAND OF HAWAII.



A, Apua. H, Honuapu. Ka, Kaaluala. Ke, Kalae. Kh, Kapoho. Kk, Kahuka. Kl, Kealakeakua. Ko, Kealakomo. Kp, Kalapana. Kw, Keaiwa. N, Ninole. Po, Pohue. Pu, Punaluu. W, Waiohinu.

In passing from Puna to Kau we took the high trail from Panau to Kapapala, a distance of about thirty miles. This whole route is a dreary wilderness, without a human habitation, a drop of water, or any thing to refresh man or beast. The track at first lies over *pahoehoe*, sand and *aa*, sprinkled with small trees and bushes; but the latter half of the way is upon open fields of lava and sand, and under a burning sun, with nothing to break his fierce rays. The road flanks Kilauea at a

distance of nine miles on the south side. At one place we rode for miles on the verge of a precipice about 100 feet high, the wide region below us, on our right, giving evidence of a vast subsidence during some former age.

As soon as we left the region of trees and shrubs, and came into the sweep of the trade-winds, the sulphurous smoke from Kilauea filled the atmosphere, and was almost stifling to horses and riders. A dense cloud of gases and smoke spread over the country, and hung along the hills and over the plains, as far as the sea-shore at Punaluu. We now came upon the great lava fields of 1823, which are studded with conical tumuli, and more or less fractured with old fissures. But the late earthquakes, and the subterranean flow of the fires of Kilauea, have made strange work of the superincumbent strata. The old road is full of yawning cracks, and impassable. We were, therefore, obliged to deflect to the left and make a detour of a long distance from the track, in order to pass this terribly rent region. Our cavalcade numbered sixteen, all natives but myself, going to Keaiwa to attend our meeting of Association; and had it not been for an expert guide who marched us north, south, east and west; who zigzagged us, taught our horses to leap fissures, and who performed a score of necessary evolutions, we could not have passed that region of marvelous fractures, and of fissures from one to twenty-five feet wide. At last we were all crossed over. This done, I took the guide, and while the rest of the company went on, we turned due south, upon the *pahoehoe*, in search of a disputed outburst of fresh lavas which some affirmed and others denied had been deposited there. After an hour's search amidst this wild sea of ancient lavas, our guide, by mounting a ridge of *aa*, descried steam in the distance, and we were soon upon a patch of warm and steaming lava 1,000 feet long, 600 wide, and four to twenty-five feet deep, with an uneven, ropy and tumulated surface. This had been thrown up from an open fissure; below it, on the same line of rents, four more patches had been thrown out, of less areas. We now traveled to the west till we found the old trail and reached the ranch at Kapapala, where I spent the night. Here I examined the effects of the 2d of April. The yards, the plains and the hillsides were full of rents and seams; and in one place the earth opened and closed again, and in closing, one side of the rent surface lapped over the other, like the flap of an amputated limb. From Kapapala onward to Waiohinu the flanking hills and spurs of Mauna Loa were torn and scarred and striated and grooved by landslides of greater or less extent. Avalanches of rocks and patches of soil, with trees, shrubs and grass had been sent down from the steep hills everywhere.

Some of the steeps had the appearance of having been plowed in deep and broad furrows, leaving wide belts of green between them, which resembled rows of cane or hedge. The general trend of the fissures, from Kilauea to Kapapala, and onward to Kahuka, in western Kau, is southwest, running under the foot-hills and spurs of the mountain, as they rise, in an advancing and retreating line of verdant beauty and lofty grandeur, on the right of the traveler passing on the main road from eastern to western Kau.

Along this whole line we find the greatest disturbances. From the fiery jets which were thrown up along this line, on the *pahoehoe*, called *Nuku pili*, and from the awful shakings and rendings all along these hills, as also, from the testimony of multitudes, that they heard hissings under ground, and thumpings as of the striking of waves under a ship, we may conclude, that Kilauea sent off her burning messengers in this direction; and that her fires united with those of Mauna Loa and burst forth in fury at Kahuka. The testimony of many, that steam issued from several points of the fissures along this line, gives still greater probability to this view.

And this is the only probable theory to account for the exit of the vast molten floods which commenced to leave Kilauea, subterraneously, on the evening of the 2d of April, immediately after the great shock, causing a subsidence in the crater which continued until the 7th of April, when the vent opened at Kahuku, some forty miles to the southwest. As there is no proof that the fires of Kilauea went into the sea at any other point, and as there is positive evidence that they went fifteen miles, subterraneously, in this direction; and as the mountain and Kilauea were relieved, and their camp-fires extinguished at the same time, it is reasonable to infer, that they were in sympathy, acted in concert, and found final vent together at Kahuka.

While at Keaiwa I made a careful examination of what has been, erroneously, called "The Mud Eruption" and "The Mud Flow." I went entirely around it, crossed it in three places, and measured it. The length is just three miles, the breadth, in the center, half a mile, and at the head, where the cleavage took place, one mile. The depth is various, averaging about six feet on the grass plains, but deepening to twenty, thirty and forty feet in some of the gorges and depressions. It is nothing more or less than a landslide, having none of the characteristics of a mud eruption. You are aware, that the hills lying back of Kapapala and Keaiwa, and sweeping around to Waiohinu and beyond, are very steep, sending down lofty and beautiful spurs upon the grassy plains below, and buttressed by

bold cones and headlands. Between these hills and the shore there is a gently inclined belt of land, four to six miles in width. On the steep highlands, above these precipitous hills, between Kapapala and Keaiwa, and in the forests of trees, ferns and jungle, the earth was terribly rent, on a line parallel to the shore, or northeast and southwest, and the face of the hills was shaken off, leaving a bold precipice, or wall, twenty to sixty feet high. The whole mass, below this wall,—earth, water, boulders, rocks, lavas long buried, trees, logs, etc.,—slid, rolled, pitched and tumbled down a steep incline—an upper terrace—until coming to a pali about 1,000 feet high, and on an angle varying from thirty to seventy degrees, it plunged down this fearful steep, and constantly gathering momentum, it rushed across the plains below by its own gravity, at the rate of more than a mile per minute. It was not mud, though there was much water in the caverns of these hills; and where this became mixed with the soils in the descent, it formed mud of course, as earth and water mixed always do. But the mass was the superincumbent strata of the hills, as the earthquake shook them off; and by sliding, rolling and plunging, under the force of gravity, all these materials were mixed up in one vast conglomerate mass. That this mass was not all mud is evident from the fact, that the whole atmosphere above and near it was filled with dry dust; and that it was not exploded by steam or gases, appears probable, not to say certain, from the fact, that no steam was seen, and no heat was evolved. The whole mass was in a natural cold state. From reports, I had supposed the eruption to have been projected from the hills by some explosive force; but a careful personal observation satisfies me, that this was an error, and that gravitation alone was the motor which propelled it, after the earthquake had rent and detached it from the parent hills. It now looks like a great plowed and harrowed field, and the taro, banana, fern and grasses are shooting up on its surface, while the natives are beginning to plant upon it.

From Keaiwa I went down to Punaluu, and thence along the shore to Honuapu. All is wreck and ruin along this coast. Churches, dwellings, yards, causeways, roads, fish-ponds, canoes, fish-nets, tackle, gear, furniture, and all things movable, were destroyed. In many places the road is sunk, or so obliterated with debris, that, without a guide, I could not have threaded my way along a shore and through villages once so familiar to me.

At Punaluu and Honuapu, I took correct measurements, on palms and on the ridges of *aa*, of the height of the earthquake waves of April 2d. The greatest height was twenty feet. The

sand-beach at Punaluu was swept out to sea by the receding waves, and the beautiful pond of cold water filled with the ocean, and apparently blotted out forever. At length, however, the sea brought back her spoils and formed another sand barrier, more than 100 feet within the old line, leaving a few pools of water inside, and protecting them from the ocean surges. But the great, deep, cool and beautiful fish-pond is not there in its normal state.

The same is true at Honuapu and all along the coast.

I spent a Sabbath at Waiohinu, and surveyed the sad ruins of the place. Landslides, rents, scars and evidences of terrible disturbances were everywhere visible. A congregation of some 250 gathered in a beautiful kukui grove and listened with great attention to the word.

On Monday the 17th of August, I started, a little after sunrise, to visit the lava eruption of the 7th–10th of April, at Kahuka and vicinity. Capt. Brown, who keeps the Kahuka ranch, and who was driven out with much peril on the 7th of April, with his wife and six children, kindly offered to accompany me. Our course was southwest. After riding three or four miles, we crossed the first branch of the flow, a high ridge of *aa* some two miles long, and lying like a vast serpent upon the beautiful pasture lands of the people. Passing this, we rode about half a mile, and then crossed another stream of similar character and dimensions. These were each about 500 feet wide and fifteen to twenty-five feet deep. The next, or third, spur we crossed was near half a mile wide and some three miles long, from where it left the trunk stream. We soon crossed a fourth branch, which resembled numbers one and two. The general direction of these lateral streams was southeast, but varying of course. Their united breadth would be about one mile. The largest branch ran toward the landing at Kaalualu, and threatened the harbor. Having passed these four branches, we rode rapidly down the open and beautiful incline, to the great precipice at Kailiki, about one mile north of Cape Kalae, and close to the shore. Here, at a height of 400 feet, we had a grand bird's-eye view of the two main streams, which had flowed parallel with the precipice and with each other, for five miles on the old *pahoehoe* fields, 400 feet below this remarkable pali, which stands as a great rampart, forming the western line of the high plateau of grazing and cultivated fields extending to Waiohinu. This pali lies nearly north and south, extending from the south cape to the base of Mauna Loa. The cooled and shining streams which lay below us are each about half a mile wide, and are *pahoehoe*. They ran smoothly into the sea, say 1,000 feet apart, inclosing an

island on three sides, about five miles long. The ocean line completes the island. On this narrow belt of land, three houses are standing near the shore, and here, also, Capt. Brown found and rescued some thirty head of cattle, after the flow had ceased. Several cattle died, and those which survived were terribly heated by the inclosing fires.

The features where the lavas entered the sea are like those at Nanawale in Puna, and like many other places. There was, probably, a small conical island formed a little distance from the shore, soon after the stream went into the water; but this soon became a cape, or headland, by the continuous pouring in and filling up between the cove and the shore. The coast line is not much extended, and the two sand coves are much smaller than those of Nanawale in 1840.

From the termini of these streams we rode up on the high precipice, having the two streams, with the long island, under us and in full view on the left, until we came to the great trunk, or parent stream, from which all the lateral branches had been sent off, the trunk somewhat resembling the palm of the hand, and the branches the spread fingers.

We visited the place where Capt. Brown's houses once stood, and where his young daughter was buried; and finally crossed over to the eastern margin of the stream, above all its lateral branches, and rode on the highlands which skirt its eastern margin for about a mile. The side hills on which we rode were covered with cinder and pumice, thrown up from the great seething river below; and all the trees and ferns were consumed or charred. At length we descended again upon the stream, which here appeared to be one and a half mile wide, and rode another half mile on its crackling surface. Here and there, there was a little heat, and white puffs were seen scattered, widely apart, on some parts of the flow.

At length we came to the point beyond which horses do not go. Here we left our beasts and proceeded one-fourth of a mile, over a horrid surface, and up a steep and shaggy precipice of fresh lavas, to the very head of the eruption. All this region shows marks of the raging volcanic fires. There is every evidence of the fury of the furnace, when it was in full blast. There may have been a great crater here, with many orifices and fiery blow-holes; but if so, they are all filled and obliterated. What we now see is a great fissure, or rent, running irregularly from the head of the eruption for two and a half or three miles, in a southerly direction. This I traced distinctly with the eye, from my high position on the great ridge where the rending and disgorgement commenced. This was in a forest, but the fires have cleared out the thicket, so

that the view is clear for four miles down the stream, taking in its entire breadth. In some places the fissure yawns twenty to thirty feet, in others it is ten, six, four and two feet. Near the head I crossed it without difficulty, and I followed it for some distance, and looked down its fearful jaws. It is hung with stalactites, and is still hot at some points. I found alum sulphur, sulphate of lime, and Glauber salts in small quantities. None of the salts are abundant, and the products and general character of the eruption are similar to our former eruptions.

The whole length of the stream does not exceed ten miles—probably it is less—and I would give the average width, by uniting all the branches, one and a half miles. The depth varies from two feet to fifty, and more. The general course is south, or nearly so.

I employed two men to measure across, but they were stopped, about midway, by an impassable fissure. This I much regretted, as time would not allow me to wait for another trial. I, however, left a request for another effort and hope to hear of success.

I returned via Kilauea, and made examinations of the great crater and its surroundings. All around the upper rim the earth is terribly rent, and immense avalanches of rocks have been detached from the walls and sent thundering down to regions below.

Before the earthquake of the 2d of April, the lavas of Kilauea burst up vertically in the bottom of Little Kilauea, and spread over the old deposit of 1832. On the night of the earthquake the fires of Pele began to be extinguished and her lavas to escape through underground passages. Consequently, a subsidence has taken place in all the central portion of the crater. The outer circumference of the pit remains intact, a well defined "Black Ledge" of 100 to 600 feet wide. The central area, or the great plateau which has been lifted up from year to year by the forces below, and without disturbing the surface, has sagged gently down, carrying with it its botanical garden of ferns and ohele bushes undisturbed. The subsidence is about 300 feet in the center, forming a great basin, or caldron, with its sides or angles of 30° to 60°. The ferns are still growing in the bottom of this basin, 300 feet below their position on the first of April.

In going to the south lake we first cross the "Black Ledge," then descend into the great basin, cross its bottom, and then rise an incline of 20° on the south side, for half a mile, when we come to Old Halemaumau. But how changed! It is a pit 500 feet deep, about 3,000 feet in diameter on its upper rim, and 1,500 on the bottom. It is nearly circular. Its

walls are, in some places, perpendicular, in others, overhanging, and in others, on an angle of 40°, but everywhere jagged and threatening. When I was there a little light, or faint blush, was seen at night, but no fire was visible during the day; but there was much smoke.

My daughter H. has prepared the outlines of a map of Hawaii, on which you will find some marks which may help you as to localities, courses, etc. We have no really correct map of the island.

I see Mr. Brigham's map has several important errors in the location of places, and in the situation and comparative sizes of lava-flows. The eruption of 1855 was immensely greater than that of 1859, but his map makes the latter much the larger.

The amount of matter discharged by our recent eruption is small compared with that of many former eruptions. The eruption was short and fierce. So rapid was the rush of the rivers of lava, that cattle, grazing on the plains, were surrounded before they were aware of danger. Some were consumed; but I saw several green islets, of two and three acres, where ten or twenty head were inclosed for days and afterward rescued. Houses were, also, surrounded by the lavas and left unscathed, and a great ridge of burning *aa* would come within twenty-five or thirty feet of a house, pass by it, cool as high as the ridge of the house, and not burn it. I visited a family of four on an island formed by the igneous flood, where they were inclosed for ten days on an area of about one acre. The burning stream came within twenty feet of their house, on one side, and yet the house stands, and they all remain in it. I asked them what they did and how they felt during those days of "fiery trial." They replied, that when they found themselves surrounded and all hope of retreat cut off, they gave themselves up to God, and continued in prayer. Many cases of escape from the fire and from the sea were marvelous, and seem like miracles.

Ere this reaches you, you will have heard of the strange tidal phenomena which occurred around our group on the 14th, 15th and 16th of August. Without any apparent cause the sea rose and fell three to six feet once in ten, fifteen, and twenty minutes, for three days. I was at Keaiwa at the time and only heard the reports. These reports vary as given by different individuals and in different localities.

These rapid and long-continued oscillations of the sea are a puzzle to us, and we wait to hear from the coast, or from some other regions, to decide whether the cause was near or remote—

whether the result of submarine eruptions, or the effect of great disturbances in distant parts.

Our shakes continue up to this time, but they are not severe.

Number of houses buried by landslide,	10
" " deaths " "	31
Houses destroyed by igneous eruption,	37
" " " influx of sea,	108
Deaths " " " "	46
Houses " " earthquake,	46

All the above disasters were in Kau.

Hilo suffered not a little in buildings and other works.

On the 8th of August Hilo and Puna were visited by a most awful and protracted thunder-storm. It commenced a little after noon and continued until midnight. The clouds rested on the earth and the whole atmosphere was surcharged with electricity. The air was like hot steam, and white streams of lightning were constantly flashing out and playing along the ground, the report coming with the flash and seeming to make the earth tremble. I was in Puna at the time, and the natives were greatly alarmed. One man went out of doors and returned immediately, saying, that the lightning looked like white hens, running on the ground around him. This electrical storm must have been excited by the volcanic action, as thunder-storms in summer are as rare with us as rain in Egypt or on the coast of Peru.

The map I send does not profess to be exactly correct, but it is sufficiently so to give you a general idea of localities.

A sharp earthquake occcurred on the 6th inst.

ART. IX.—*Geographical Notices;* by D. C. GILMAN.

I. NOTES ON CHINA, BY REV. W. A. P. MARTIN, D.D.

A RECENT number of the Journal of the North China Branch of the Royal Asiatic Society, contains an account of an overland journey from Peking to Shanghai, made in 1866, by Rev. W. A. P. Martin, D.D. This distinguished Chinese scholar, has been for many years a missionary of the American Presbyterian Board, and is now by appointment of the Imperial authorities of China, one of the Professors in the newly established University of Peking. The article referred to discusses four topics; I, the imperial road leading south from Peking; II, the present condition of the Jews in Honan; III, the navigation of the Yellow River; and IV, the central section of the Grand Canal.

From this communication we gather such facts as are of most interest to scientific readers.

1. *The Si-Shan Hills.*

Passing south from Peking to Kai-fung, the writer thus refers to the metallic deposits of the Si-Shan hills, which meeting him outside the gates of Peking run parallel to his course for nearly four hundred miles. Their highest peaks were "crowned with snow (in February and March) and glittering like a thousand gilded domes, their rugged sides resembling the wave-worn shore of a long retired ocean."

"Silver, they certainly do contain, but the mines of Shan-si, whether from defective engineering or other causes, are no longer remunerative, and have ceased to be worked. Of gold we have no notice; but the "black diamond" is found there in rich deposits, and along with it an abundance of iron, the most precious of all metals. Iron founderies are in operation in at least two districts—one near Peking and the other in *Hooh-lu-hien*, about two hundred miles to the south. As we passed the latter place, we met a vast number of carts conveying its productions to all parts of the province. These ranged from kitchen utensils up to salt boilers, five or six feet in diameter. They appeared to be well executed and the metal of good quality.

"Of coal deposits, there seems to be a continuous chain, extending from the verge of the Mongolian plateau to the banks of the Yellow River. In the vicinity of Peking there are beds of both bituminous and anthracite, but at other points, I met only with the latter variety. With the exception of places near the Hwangho, it is transported mainly by land carriage—near Peking on the backs of camels, further south on mules, donkeys and wheel-barrows. The consequence is, that while at some points, it is cheap and abundant, at intermediate places it becomes so costly that the people are obliged to burn reeds and millet-stalks or glean a scanty supply of fuel from their stubble fields.

"Here then, on the line of this imperial road, and along the base of this range of hills is the track for *the first grand trunk railway in the Chinese empire*. Not only would it find close at hand iron for its rails and coal for its motive power; but the carriage of coal and iron to all the cities on the line including Peking and Tientsin would constitute one of the richest sources of its revenue. With Ta-ku for one terminus and Kai-fung for the other, it would pass through the capital of the empire, through two provincial capitals, six *fu* cities, and an indefinite number of *chows* and *hiens*.

"Between these places the amount of local travel is immense. At some points I estimated the number of vehicles passing in the course of a day at two hundred, employing from four to five hundred mules; while caravans of pilgrims mounted on camels were flocking to the shrines of Shan-si as the Hindoos do to those of Benares. The supposed railway would soon supersede these slow and painful modes of locomotion."

2. *Recent Changes in the course of the Hwangho.*

Dr. Martin crossed the Hwangho, the lesser one of the great twin rivers of China, at three points;—first near Kai-fung, where it still continues in its old channel; again at Tsing-kiang-pu, where he walked dry shod over the place where Lord Amherst's junks offered incense to secure a favorable passage, and third near Tung-ping-chau, where the river was hastening in its new course toward the Gulf of Chili. He thus describes the wonderful change which has thrown *five hundred miles of sea coast* between its present and its former embouchure.

"According to the best information I was able to collect, the breach that opened the new channel occurred near *E-fung-hien*, thirty or forty miles to the east of K'ai-fung-fu. From that point, washing the city of K'au-ching, it flows north passing under the walls of Ts'au-chou-fu, as far as Fan-hien, where it spreads into a lagoon some thirty *li* in width. I passed near this place, and should have crossed the river here but for the ice that had formed on the lagoon. Turning in an easterly direction it intersects the Grand Canal at *Chang-ch'iu-chen*. It was at *Li-lan-k'iau*, a little beyond this place that I crossed it—it had there diverged from the canal to the distance of fifteen *li*. A stone bridge that gave name to the locality, and which in former years sufficed to carry passengers over a small tributary of the Ta-ts'ing, was lying in ruins, the advent of the Hwang-ho having tossed it aside with little ceremony. From this point, it not only usurps the bed of the Ta-ts'ing but obliterates its very name—the natives everywhere speaking of that startling phenomenon, the 'Coming of the Yellow Waters.'

"As to the cause of this phenomenon, we are left very much to conjecture. Superstition discovered a mysterious relation between the outbreak of the Taiping rebellion and the behavior of the unruly stream in refusing to pay tribute to the Eastern ocean, bursting over all bounds and pouring its waters into what the natives call the Northern Sea. They view it only in the light of a portent; but the alleged relation is not to be set aside as altogether imaginary. Dr. D. J. Macgowan, who in a commu-

nication to the *North-China Herald* first drew public attention to the remarkable change in the Yellow river, quotes authority to show that 'in the latter part of 1852 the people of *Hwai-ngan* found the river fordable, and in the spring of the ensuing year travellers crossed it dry shod.' Mr. Wade in the Parliamentary Blue Book for 1859–1860 cites a Chinese document to the effect that by an inundation in 1855 'the north bank of the river in Honan was carried away and the river ceased to flow.' Now it was just between these dates that the Rebel invasions of the northern provinces took place; and what more natural supposition can we make than that the *Ho-tuh* or Superintendent of the River-works, who has under him a force of 64,000 men, on a *quasi* military footing, should have found other employment for his 'navvies' on the approach of the enemy, and neglected the river at a critical juncture? He may even have employed the impetuous stream as means for checking their advance. A rumor became current at the time, that many rebels had been drowned in consequence of the river breaking its banks. That this outbreak was the result of neglect occasioned by the rebel panic is highly probable; but the military use of the river which I have just hinted at is not without a precedent. The Chinese are as well acquainted with this method of extinguishing an enemy as were the heroes of the Dutch Republic. Not to speak of other instances, K'ai-fung-fu has in this way been subjected to at least three destructive inundations. Once by the forces of *Ts'in* for the purpose of dislodging the prince of *Wei* who held his court in what was then called *Ta-liang*; once by the Mongols in their conflict with the Sung dynasty, and again by a general of the Ming's with a view of destroying a body of rebels who were laying siege to the city. The whole population of the city fell victims to the miserable stratagem, and Chinese historians charge the cruel act to the rebel *Li-tse-ching*; but we prefer the contemporary testimony of Jesuit missionaries.

"It is not perhaps generally known that the Yellow river in that immense departure from its late channel, which excites the astonishment of the present age, is only returning to a long forsaken pathway. The highlands of Shantung rise like an island from the level of the great plain, and it appears from Chinese records that the restless river in finding its way to the sea has oscillated with something like periodic regularity from one side to the other of this promontory, and at two epochs flowed with a divided current converting it into an immense delta. These vagaries are minutely traced in a hydrographical work published under the patronage of K'ang-he. From this we learn the curious fact that the river divided its waters be-

tween the two principal channels, and insulated the highlands of Shantung for a period of 146 years, and that it was not till the reign of the Mongols about 500 hundred years ago, that it become settled in its southern bed. The writer concludes with the expression of an earnest desire that the troublesome stream may be induced to return to its northern course. After the lapse of two centuries his wish is now gratified."

The reader who is interested in studying the recorded changes in the Hwangho delta (which are probably without a parallel in the history of physical geography) will find a very instructive series of illustrative diagrams, eleven in number, appended to Professor Raphael Pumpelly's Observations on the Geology of China, Mongolia and Japan, published in 1866 by the Smithsonian Institution. (Wash., 162 pp. 4to.) The text of Mr. Pumpelly gives a synopsis of the Chinese history respecting these remarkable alterations in the river bed. Prior to these recent changes the subject had been ably discussed by M. Edouard Biot in the *Journal Asiatique* [iii], tome 14, p. 152 [iv], tome 1, 453, and [v] tome 4, 408. Mr. Pumpelly's and Mr. Bickmore's narrative of observations on the Delta were printed in this Journal for March, 1868.

3. *The Great Imperial Canal.*

Dr. Martin thus continues: "The canal, where I crossed it, to the south of *Tung-ch'ang-fu*, was nearly dry, and I had not been able to learn whether it was in a working condition above *Ts'ing-kiang-p'u*. From Kiuhfan to this place, it was accordingly my intention to proceed by land; but my cart driver taking alarm at rumors of rebels refused to advance, and I was compelled to seek for some other mode of prosecuting my journey. The canal was suggested, and I made my way in that direction slowly, painfully toiling on, now on foot, now on a wheel-barrow, anon mounted on one of the imperial post horses, or seated in a mandarin carriage. At length ascending a hill I saw the *Weis-han* lake spreading its silvery expanse at my feet. Embosoming an archipelago of green islands, and stretching far away among the hills—to my eye the scene was too pleasing to be real. I distrusted my senses and thought it a *mirage* such as often before had cheated my hopes with the opposition of lake and stream; and when my guide assured me that it was no deceptive show, I gave way to transports not unlike those in which the Greeks indulged, when escaping from the heart of Persia they caught a distant view of the waters of the Euxine.

"Taking passage at the foot of the lake I glided gently down with the current, and reached *Chin-kian-fu*, a distance of 900 *li* in less than a week.

"Through this portion of its course the canal deserves the appellation of 'Grand.' For the first half, extending to *Ts'ing-kiang-p'u* it varies from eighty to two hundred feet in width. Seething and foaming as it rushes from the lake and rolling on with a strong current through the whole of this distance it has more the appearance a natural river than of a canal. Near Ts'ing-kiang it parts with enough of its waters to form a navigable stream which enters the sea at *Hai-chau.* Beyond this point, where it intersects the old bed of the Yellow river, its waters are drawn off by innumerable sluices to irrigate the rice grounds, until about forty feet in breadth and four in depth. Recruited, however, by a timely supply from the *Kav-yu* lake, it recovers much of its former strength, and flows on to the Yang-tsze-kiang with a velocity that makes toilsome work for the trackers.

"To what extent the canal may be practicable for steam navigation is a question not without interest; and my mind had been occupied with it for some days, when I happily had the opportunity of seeing it subjected to the test of experiment. Just off the city of Kau-yu, where the canal reaches its minimum depth, I met the Hyson, a well known tug-boat from Shanghai, towing a flotilla of war junks. She would be able to reach the city of *Ts'ing-kiang-p'u,* but not to go beyond on account of the locks or water-gates, some of which are only twelve feet in width.

"As the canal now is, propellers of three feet draught and ten feet beam, making up in length what they lack in other dimensions, might drive a profitable trade between *Chin-kiang* and *Tsi-ning-chau* a distance of 1,200 *li*; but the utility of the canal would be greatly enhanced by adding a lock or two in the shallower portions and increasing the breadth of those that now exist so as to admit the passage of larger vessels. A little engineering at its point of intersection with the new course of the Yellow River would supply an abundance of water to a portion that is dry, making its facilities for junk navigation equal to those of its best days; and it would then be possible for steamers of the class that were lately employed in penetrating to the silk districts of the interior, to make inland voyages from Shanghai nearly to the gates of Peking."

II. Arctic Explorations.

1. *European Expeditions.*

The German North Polar expedition, in which Dr. A. Petermann has been so deeply interested, has returned without having achieved the highest results. The party came back to Bremen early in October last, and Capt. Koldewey has already

published an outline of his summer work. Some interesting observations were made on the northern coast of Spitzbergen, in Henlopen Straits, and on the southern shore of North-east land. The snow-covered peaks of Gillis land were seen by telescope from the deck of the "Germania." The farthest point reached was 81° 5′ N. lat., in long. 16° E., on the evening of Sept. 14, when progress was stopped by pack ice in every direction but the south. No doubt the observations of the party will add many interesting details to our knowledge of the North Atlantic, and the leaders are already desirous of making a new voyage in a better vessel propelled by steam.

So much enthusiasm was shown by Dr. Petermann in projecting this expedition, and in arranging for its departure, that the scientific world will share in this disappointment, and will wish for him ample means to renew and carry forward his Arctic researches.

The expedition projected by M. Lambert under the auspices of the Emperor of the French and a committee of learned men, is to enter the Polar Seas by Behring's Straits. A subscription of 600,000 francs is called for as the material support of this undertaking.

The Swedish Arctic Expedition, according to a letter from Prof. Nordenskiöld (dated at Kobbe Bay, on the northwest coast of Spitzbergen, Sept. 16), went as far north as 81° 9′, where ice stopped it at the end of August.* A week later the sea was clear, and from one of the highest peaks of Parry Island (80° 40′ N.E. of "North East Land") traces only of ice further northward could be seen. The exploring steamer then made again for the north, whether to pass a winter in the ice or not is uncertain. The coal ship returned to Sweden with the collections already made.

2. *Renewed recommendation of Smith's Sound, by Dr. I. I. Hayes.*

Meanwhile, Dr. I. I. Hayes (whose zeal in the prosecution of Arctic researches has never languished, though his plans for another expedition were interrupted by the late civil war) has delivered a lecture before the American Geographical Society of New York (Nov. 12, 1868), in which he proposes the renewal of explorations of the North Polar regions, and makes a statement of the grounds on which he bases his expectations of a successful enterprise. He is very clear and forcible in the advocacy of what may be termed "the American route" northward by the way of Smith's Sound. This is the course (as our readers will remember) by which he discovered in 1854 and revisited in 1861, *the most northern land* yet known on the

* Later reports say that the highest latitude attained in open sea was 82° 40′ N.

globe, and it is the course which he had proposed to follow in 1862 if a third opportunity of exploration were offered to him. While he does not now expressly pledge himself to a fresh expedition, he says that he is no less earnest than formerly for the opportunity to conduct a party over the to him familiar course, and "to try conclusions with his old foe, the Smith's Sound Ice." Whether the American Geographical Society, which is already, through Mr. Henry Grinnell and others, so honorably identified with the progress of Arctic research, will render new aid for these proposed researches remains an open question.

In connection with a review of the history of Arctic discovery, Dr. Hayes examines briefly the comparative advantages of the four proposed entrance routes, namely: 1. *Smith's Sound*, which he prefers; 2. *Behring's Straits*, advocated by Gen. T. L. Kane, and Capt. S. Bent, U. S. N., and adopted also by the French expedition under M. Lambert; 3. Between Spitzbergen and Greenland, the route proposed by Dr. Petermann; and 4. Between Spitzbergen and Nova Zembla, which Dr. Hayes regards as least promising of all.

Two considerations are urged in preference of Smith's Sound: land as a base of operation, and the opportunity to colonize a party of hunters and natives as a permanent support.

The eloquent terms in which Dr. Hayes pleads for a renewal of northern discoveries, and the intrepid spirit which animates him, are well fitted to awaken new zeal in the public and to enlist the support of men of wealth. Dr. Hayes's address has been printed in a pamphlet form by the Society to which it was delivered. (N. Y., 1868, pp. 44.)

3. *Capt. Long's Discovery of "Wrangell's Land"* (70° 46′ N. lat. and 178° 30′ E. long.). *His opinion of the Behring's Strait route.*

Capt. Thomas Long, of the bark "Nile," who has been well known in years past as a most successful whaling master, published rather more than a year ago a letter addressed to Mr. H. M. Whitney, the editor of the *Pacific Commercial Advertiser*, dated Nov. 5, 1867, giving an account of his discovery of land hitherto unknown in the Arctic Ocean, and Capt. G. W. Raynor, of the ship "Reindeer," at the same time published his notes of observations of land in the same vicinity. Both writers refer also to the testimony of Capt. Bliven who, while cruising near *Herald* Island (71° 20′ N. lat., 175° W. long.), eight miles southeast of Wrangell's Land, saw the mountain ranges extending northwest as far as the eye could reach. As the position of this land is attracting much attention, we place on record the essential portions of the two letters.

Capt. Long writes :—

HONOLULU, November 7, 1867.

During my cruise in the Arctic Ocean this season, I saw land not laid down on any charts that I have seen. The land was first seen from the bark Nile on the evening of the 14th of August, and the next day at 9½ o'clock A. M.; the ship was eighteen miles distant from the west point of the land. I had good observations this day, and made the west point to be in lat. 70° 46′ N., and long. 178° 30′ E.

The lower parts of the land were entirely free from snow, and had a green appearance, as if covered with vegetation. There was broken ice between the ship and land; but as there were no indications of whales, I did not feel justified in endeavoring to work through it and reach the shore, which I think could have been done without much danger.

We sailed to the eastward along the land during the 15th and 16th, and in some places approached it as near as fifteen miles. On the 16th the weather was very clear and pleasant, and we had a good view of the middle and eastern portion of the land. Near the center, or about in the longitude of 180°, there is a mountain which has the appearance of a distinct volcano. By approximate measurement I found it to be 2,480 feet high. I had excellent observations on the 16th, and made the southeastern cape, which I have named Cape Hawaii, to be in lat. 70° 40′ N., and long. 158° 51′ W. It is impossible to tell how far this land extends northward; but as far as the eye could reach, we could see ranges of mountains until they were lost in the distance, and I learn from Capt. Bliven, of the bark Nautilus, that he saw land northwest of Herald Island as far north as 72°.

Capt. Raynor writes :—

HONOLULU, November 1, 1867.

I send a short account of a large tract of land lying in the midst of the Arctic Ocean, hitherto but little known. This island has heretofore been considered to be two islands, one of which has been marked on the English charts as Plover Island, which is laid down to the W.S.W. of Herald Island. The other is simply marked "extensive land with high peaks." On my last cruise, I sailed along the south and east side of this island for a considerable distance three different times, and once cruised along the entire shore, and by what I considered reliable observations, made the extreme southwest cape to lie in N. lat. 70° 50′, and E. long. 178° 15′. The southeast cape I found to lie N. lat. 71° 10′, and W. long. 176° 40′. The south coast appears to be nearly straight, with high rugged cliffs and entirely barren. The northeast coast I have not examined to any extent, but it appears to run from the southeast cape in a northwesterly direction for about fifteen or twenty miles, and then turns to the north and northeast. I learned from Capt. Bliven that he traced it much further north, and has seen others who traced it to the north of lat. 72°. I think there is no doubt that it extends much further to the north, and that there is

another island lying to the east of it, say in long. 170° W., and to the northwest of Point Barrow, with a passage between it and the land I have just described. My reason for thinking so is this: We always find ice to the south of the known land, further to the south than we do the eastward of it. The current runs to the northwest from one to three knots an hour. In the longitude of 170° W. we always find the ice barrier from fifty to eighty miles further south than we do between that and Herald Island, and there is always a strong current setting to the northwest between those localities, unless prevented by strong northerly gales (for in such shoal water as the Arctic Ocean the currents are changed easily by the winds), which would indicate that there is a passage in that direction where the waters pass between two bodies of land that holds the ice, the one known, the other unknown.

I would add that the southwest cape of this island, described above, lies twenty-five miles distant from the Asiatic or Siberian coast.

More recently, Capt. Long has announced this discovery in a letter to the Geographical Society of Paris, which is printed in their journal for Sept., 1868. He has also written for the *Pacific Commercial* a letter strongly advocating the Behring's Strait route for Arctic discovery. For copies of these very interesting letters of Capt. Long we are indebted to a merchant in New London, Conn., Henry R. Bond, Esq., who has watched very closely the progress of all the Polar discoveries. He informs us that the summer of 1868 has been what is termed "an icy season" in the Arctic Ocean, and that the whaling fleet has probably been unable to go as far north as in 1867, which was a peculiarly open season.

In view of all that is said at home and abroad on this subject, we copy the statement of Capt. Long in respect to the Behring's Strait route for Arctic research.

The *route* I would recommend as the best would be to follow the Asiatic shore from Behring's Strait as far as Cape Kekurnai or Cape Schelagskoi. The ice melts earliest near the shore, and the melting of the snow upon the land forming innumerable streams of water, impels the ice from the shore, leaving an open lane of water near the shore, through which a ship can pass without difficulty, especially when assisted by steam in calms and adverse winds. After passing Cape Jakan, there being no land to the north, the ice is driven from the shore by these streams and scattered in fragments in the open sea seen by Wrangell, with sufficient openings for the safe navigation of a ship. In the month of August, 1867, the bark *Nile* passed over a position within ten miles of the point where Wrangell saw the open sea in March. From some point between Cape Kekurnai and Cape Schelagskoi the course would be from north to northwest, as the ice would permit, until north of the Läächow Islands, when the ef-

fects of the current from the rivers of Northern Asia would be sensibly felt. From thence a course directly toward the North Pole or Spitzbergen, as would appear most feasible, should be pursued.

After getting to the north of the Läächow Islands, should a vessel be obstructed by ice, the current, though not as strong as that found north of Spitzbergen and in Baffin's Bay, *would eventually carry the vessel through one of these channels into the Atlantic.* In the event of any disaster to the vessel, the chances for the preservation of the lives of those on board are much greater than by the route east from Behring's Strait, as from the river Kolyma to the westward, Russian settlements are found near the mouths of all the rivers, where assistance can be procured.

Another route by which this voyage can be accomplished is to follow the shore from Behring's Strait to the mouth of the river Lena, and from thence directly north beyond Cape Sievero Vostoschni; from thence to the westward toward Spitzbergen. After passing the mouth of the Lena, a vessel would receive assistance from the current of this river and the other rivers between the 105th and 140th meridians.

The effect of those large rivers in impelling the ice from the land was seen by Franklin in his expedition from Great Bear Lake in 1826 down the Makenzie river, and along the shore toward Point Barrow. In this expedition, he reached the longitude of 149° W., with but little impediment from ice on the 15th of August. At this point he determined to return. His associate, Dr. Richardson, proceeded to the eastward with another party as far as the Coppermine river, without any difficulty. Franklin says that the natives informed him that, from the top of the hills at the mouth of the Makenzie river, no ice was to be seen for two months of the year, i. e., in August and September, showing the powerful influence of these rivers upon the ice. It was the current from this river and its thermal influence which enabled McClure to reach Banks's Land, and had there been other large rivers to the eastward, with no land to obstruct their discharge northward, would have enabled him to make the passage between one ocean and the other.

The months of August and September are, I think, the best months for explorations along the shores of the Arctic Ocean. American whalers have passed to the eastward of Point Barrow, and taken whales as late as the 15th of September, seeing no ice to the northward except in the immediate vicinity of Point Barrow. Whales have also been taken as late as the 12th of October in lat. 71° N.

Deshnew, it is certain, in 1648, sailed from the mouth of the Kolyma along the coast of Asia, and passed through Behring's Strait to the Anadyr river. The account of this voyage, though vague and uncertain in regard to its details, yet established the fact of the separation of the continents of Asia and America.

Shaularow, Billings, and others attempted exploration along this coast, but were unsuccessful, and some of them perished in

their attempts. When we consider the scanty facilities and rude structure of their vessels, we cannot wonder at their failure.

With our modern improvements in the structure of vessels and appliances for propelling them, what to the navigators of 200 years ago appeared possible, should to us appear and be of easy accomplishment.

That a vessel properly fitted for the purpose can winter in safety at almost any point along the shore, is proved by the experience of Capt. Collinson, in the *Enterprise*, who twice wintered eastward of Point Barrow, once at Camden Bay, where there is no protection from the north, except the ice which may be grounded seaward from the vessel.

That the passage from the Pacific to the Atlantic Ocean will be accomplished by one of the routes which I have indicated, I have as much faith in as I have in any uncertain event in the future, and much more than I had fifteen years ago, in the success of the Atlantic Telegraph.

In conclusion, I submit these remarks to the public, and while deprecating criticism on any verbal inaccuracies, would invite discussion in regard to the views advanced, or the feasibility of the routes proposed. Although this route will be of no great importance to commerce as a transit from one ocean to the other, yet could the passage along the coast as far as the mouth of the Lena be successfully made every year, (which I think probable,) it would be of great benefit in developing the resources of Northern Siberia. Yours truly, THOS. LONG.

Honolulu, January 15, 1868.

4. *Hall's Search for Sir John Franklin.*

The newspapers have recently published some apparently fresh reports respecting Capt. Hall's search for Sir John Franklin. As some of these reports were quite inaccurate and have led to confusion, we have obtained the following trustworthy information from a correspondent in New London, Henry R. Bond, Esq., already referred to as a merchant interested in every thing which pertains to Arctic discovery.

These reports were brought by Dr. Goold, steward of the whaling brig "Isabella," which arrived from Cumberland Inlet in September last. The Isabella was in Hudson's Bay in August, 1867. This is the date of Hall's letters to Mr. Henry Grinnell of New York, which were received and published more than a year ago; so that the Isabella's report contains no new information.

No whaling vessels remained in Hudson's Bay last winter. Two have gone there this summer, and will probably pass the present winter there. It is not likely that there will be any further news from Hall until such vessels return next August.

"The whaling master," continues our correspondent, "who last saw Hall, has just left my office. He reports that the latter

intended if his expedition was successful to go down to York Factory, one of the Hudson's Bay Company's posts, in the summer of 1868, and sail thence to England if opportunity offered. If he had done so, it is time to have heard of his arrival in England. Hall had a party of five men with him all armed, and he proposed to fight his way to King William's Land if the natives opposed him."

III. Report of Hon. A. Campbell on the Northwest Boundary.

Some time since we called attention to the interesting maps of the regions adjacent to the disputed boundary of the United States and British possessions in the Northwest, published by Archibald Campbell, Esq., U. S. Boundary Commisioner. His Report, which has since been printed (Washington, 1868, 270 pp. 8vo), contains much geographical information, especially respecting San Juan and the neighboring islands in dispute (pp. 128–142). The document and the map together are a very important contribution to our knowledge of a region in respect to which we were nearly plunged into war. We make a single extract descriptive of the San Juan island, regretting that we have not room for a more complete synopsis of this important document.

San Juan Island is the most western of the Haro group, and has an area of about fifty-four square miles. Its greatest length is about fourteen and a half miles; its general shape being very irregular, the width varies at different localities; its widest part is about six and a half miles. Low ranges of hills trend along its eastern and western shores, those on the western side being the highest, Mount San Juan, in this range, having an elevation of about one thousand feet. These ranges slope out toward the north, and there are no elevations of any consequence on the northern shore of the island. Between these hill ranges, near the center of the island, lies a basin-like country, gently undulating in its character. There are extensive prairies in several localities, and from the south end of the island to within a short distance of its northern extremity flocks can feed on green grass almost throughout the year. The greatest amount of arable land is found within the southern third of the island.

Bellevue prairie, situated on the lower end of the island, is about two miles long by half a mile wide. Oak prairie, which takes its name from the groves of oak scattered over it, containing about one thousand acres, is bounded on the north and west by the hills along the west shore that extend across the island at its greatest width. Some of these hills are grassy to their summits, while others are more or less timbered. Immediately north and west of these hills lies a beautiful valley, stretching toward the north end of the island. The southern end of this valley contains several

hundred acres of meadow land, but on the north it is heavily timbered. The land contained in it is all apparently fertile, and around it the hills are covered with a luxuriant growth of grass. This valley lies immediately opposite to Henry Island and adjacent to good harbors.

The northern end of the island contains much good land, now covered by a heavy forest, but when divested of this it can be brought into profitable cultivation. In this region there is a grove of large cedars very valuable for lumber. One-third of the entire area of this island, or about twelve thousand acres, is well adapted to cultivation, and nearly all the remainder to pastoral purposes. The soil of the arable portions is excellent, with the exception of Bellevue prairie, which is somewhat gravelly.

Upon this island are at least four beautiful lakes. From some of these flow rivulets of sufficient size and force to produce good water power: but as yet there are no inducements for the erection of mills, as the lumber of the adjacent shores of Puget Sound is superior to that of the island, the latter having all more or less suffered from frequent conflagrations; but in a few years more, when the husbandman shall begin to receive returns for his labors in rich crops of grain, some of these sites may be selected for erecting mills to prepare the produce for distant markets.

A circumstance of great importance, in connection with this island, is the existance upon it of extensive deposits of limestone. It is to be found near the southern end, in the vicinity of the Hudson's Bay Company's station. On the western shore, near the base of Mount San Juan, immense masses raised up into perpendicular walls are seen at several localities, covering an area of many acres. The northeastern corner of the island is composed of an extensive ledge of the same material. A very small island, (O'Neal's) lying close to the northeast end of San Juan Island, containing only a few acres, is composed almost entirely of limestone. Tested by acid and burning, it proved to be of a superior quality. It exists in sufficient quantities not only for lime, but might be profitably quarried for building stone. The value of these deposits can better be appreciated from the fact that up to the time of the discovery of limestone on this island it was not known to exist at any point on Pugent Sound, within United States territory, and for building purposes it was necessary to procure all the lime used, from California or Vancouver's Island.

In the vicinity of the southern end of the island are, perhaps, the best fishing grounds on Puget Sound. Great quantities of halibut, codfish, and salmon, are taken by the numerous tribes of Indians who, at the proper season, resort to this vicinity for the purpose of fishing. The Hudson's Bay Company were formerly in the habit of putting up at this place from two to three thousand barrels of salmon alone, which were bought from the natives. Persons supplied with the proper appliances for carrying on a fishery would find it a very profitable vocation.

At the southern end of the island there is a large bay, known as

Ontario Roads, where vessels are well protected from the prevailing storms of this region. The water near the shore is not deep, and should it ever be desirable to build a wharf at this point it would require one several thousand feet in length to reach three fathoms at low water. The entrace to this roadstead from the Straits of Fuca is through a very narrow but deep channel, known as Little Belt Passage, separating this island from Lopez Island. It is a very convenient and favorite resort for vessels escaping from storms which often in winter season, very suddenly arise on Fuca Straits. On the northwestern end of the island are several bays, well protected by Henry Island, forming good harbors for vessels of light draught.

IV. Prof. Cook's new Map of New Jersey.

We have received from Prof. Geo. H. Cook, of Rutgers College, a reduced copy of the new Geological map of New Jersey. The geographical information is compiled and drawn by G. M. Hopkins, C. E., from data furnished by the State Geological Survey, the U. S. Coast Survey, and other official and private sources. The coasts, the river courses, the civil boundaries, the roads, and other horizontal features of the country are elaborately delineated. The elevations or vertical features are not so distinctly brought out. As the reproduction of a larger map by a new photographic process, the map is exceedingly interesting to cartographers. The copies we have seen are printed in a single tint; others colored geologically are probably published.

V. C. C. Parry's Report on the Physical Geography of the Kansas Pacific Railway Route. (35th Parallel.)

Dr. C. C. Parry, Geologist of the Survey, has published a preliminary report (Philadelphia, 20 pp., 1868) on the route from the Rio Grande to the Pacific Ocean, of the Kansas Pacific R. R. We make the following extract.

All the features of "the Plains" are changed on reaching the great barrier constituting the eastern outlier of the Rocky mountain range: instead of uniform slopes, the elevations exhibit abrupt rocky declivities; the valleys are cut deeply through the rocky strata, forming chasms and prolonged cañons; the sheltered recesses, irregular character of soil, and more abundant moisture, favor the growth of trees and shrubbery, while the variety of rock exposures, including volcanic, metamorphic and sedimentary, serve still farther to vary the general features of the country.

On crossing this first range of mountains near the line of the 35th parallel, at an elevation of about 7000 feet above the sea, we descend into the valley of the Rio Grande, 2000 feet lower, and from this point westward to the Colorado, a distance of 575 miles,

the principal drainage is to the south, the intervening water-sheds between the different valleys, presenting ridges of moderate elevation, as exhibited in the general profile of the surveys.

The region under special consideration, comprises three very distinct sections, which may be briefly referred to in regular order, proceeding westward from the Rio Grande.

1st. From the Rio Grande to the Colorado of the West.

2d. From the Colorado to the summit of Tehachapa Pass in the Sierra Nevada.

3d. The western slope of the Sierra Nevada, descending into the Tulare valley, and thence over the Californian coast range or down the Tulare and San Joaquim valleys to the Pacific.

I. *From the Rio Grande to the Colorado of the West*, the country presents the character of a vast upland, crossed by a succession of mountain ridges, and basin-shaped valleys, interrupted by the product of recent volcanic eruptions in the form of extinct craters, cones, and streams of lava, which have overflowed and buried up the lower sedimentary rocks. The principal mountain axes exhibit a granitic nucleus, which at certain points is exposed to view in irregular mountain ranges, trending northwest and southeast, and constituting the general frame-work of the country, as exhibited in the Sierra Madre, the Mogollon range and the Pinaleno mountains of Central Arizona. Intermediate to these is the great table-land or *mesa* formation of Western New Mexico and Eastern Arizona, comprising the sedimentary strata of Triassic and Cretaceous rocks, which spread out into broad uplands, abruptly terminated by steep mural declivities, bounding valleys of erosion, or presenting isolated butes and fantastically castellated rocks, that serve to give a peculiar aspect to the scenery The principal foci of extinct volcanic action are represented by the elevated cones of San Mateo, and San Francisco, attaining an elevation of over 12,000 feet above the sea, whose alpine slopes, reaching above the timber line, present in their covering of snow, the only wintery feature pertaining to this latitude.

It is in the eastern section of this district that we meet with the most populous and flourishing of the interesting tribes known as Pueblo Indians; here they secure not only defensive positions for their towns on the tabled summits of isolated hills, but also fertile valleys adjoining, suited to their rude agriculture, and a wide scope of grazing country, limited only by the necessity of protection from the thievish and roving Navajo and Apache.

What is known as the Navajo country, extending still further to the west and north, comprises a similar character of broken country with fertile valleys, grassy slopes, and deeply sheltered cañons, especially adapted to their mode of life as nomadic and at the same time partially agricultural; still, better suited, however, to the wants of an energetic civilized community, who can properly appreciate the advantages of a healthful climate, combined

with a useful variety of soil, and that picturesque beauty of scenery, which adds such a charm to rural life.

In passing to the valley of the Colorado, we descend by a succession of irregular mountain ranges and basin valleys, becoming more arid as they reach a lower elevation, and finally passing into the valley of the Colorado, characterized by its bare mountain ranges, desert uplands, and broad alluvial bottoms, supporting their peculiar semi-tropical vegetation.

II. *From the Colorado to the summit of Tehachapa Pass in the Sierra Nevada.*—After leaving the valley of the Colorado, and crossing the first range of mountains, bounding the valley on the west, we come upon that peculiar section of country properly characterized as the southern continuation of the Great Basin. In all its external features of isolated mountain ridges, separated by stretches of desert plain, and valleys with intermittent flowing streams, terminating in saline or fresh water lakes or sinks; in its numerous dry water-courses or *washes*, which convey the product of summer rains, in sudden floods to the lower plains, it is an exact counterpart of the settled mining district of Nevada, characterized, however, by milder winters and greater summer heats. It is also much narrower in its eastern and western extension, the entire desert tract being comprised within little over 200 miles, the valley of the Mojave occupying in its easterly course more than half of the distance.

On reaching the eastern slope of the Sierra Nevada, here much reduced in elevation, and affording a number of practical railroad passes, we reach a well-watered country with frequent springs and small water-courses flowing from the adjoining mountains, and toward the dividing ridge, good grazing and agricultural lands are interspersed with groves of timber, presenting all the desirable features of an inhabitable country.

III. *The western slope of the Sierra Nevada, descending into the Tulare valley, and thence across the coast range of California, or down the Tulare and San Joaquim valleys to the Pacific.*—This section of country comprising the southern extension of the great valley of California, is too well known to require any detailed description.

Further information respecting this region is given under seven heads, viz: Climate, Supply of Water, Vegetation, Adaptation to Agriculture, Minerals, and Facilities for railroad connections. The recapitulation is as follows.

The general comparative advantages of this trans-continental route along the 35th parallel may be thus briefly summed up:

A salubrious climate favorable to health and activity, accessible to the moist southerly currents, while at the same time protected from the severe northern blasts, receiving along the higher elevations precipitation of rain and snow sufficient to favor the growth of natural forests and upland grasses, without forming any obstruction to winter travel.

A pleasant variety of atmospheric temperature, connected with differences of elevation or exposure in closely adjoining districts, which can be selected to suit the requirements of the season, or the particular taste of individuals.

An agricultural capacity that in its proper development can be made ample to supply the prospective wants of this region, and in the production of fruits and garden vegetables, can afford the delicacies that enter into the essential wants of civilized communities.

A pastoral region unequalled in the extent or quality of its grasses, which in adjoining districts keeps up a constant supply of nutricious fodder through the year, requiring only the light labor of herding to secure the remunerative returns of this branch of industry.

A mining region yet undeveloped but sufficiently known to be characterized as second to none on the continent in the extent and variety of its mineral products, only waiting for the facilities of railroad transportation to invite and retain permanent capital and industrious labor.

A location of route which presents the special advantages of a main trunk line in being naturally connected with adjoining rich districts that will thus seek an outlet by branch roads to central commercial points.

VI. Early American Maps. Discovery of an Important Manuscript of Hakluyt.

Rev. Dr. L. Woods, lately President of Bowdoin College, in a recent visit to Europe, secured through Mr. J. G. Kohl reduced copies of thirty-two early maps relating to the discoveries on the northeastern coast of America, during the first half of the sixteenth century. They are to be reproduced in the forth-coming volume of the collections of the Maine Historical Society, probably in the course of January, 1869.

He has also had the good fortune to discover among the manuscripts of Sir Thos. Philips, at Cheltenham, a manuscript of Richard Hakluyt, drawn up in 1584 by request of Sir Walter Raleigh, and giving an account of the results of the "Western Discoveries, lately attempted." Dr. Woods was allowed to bring home with him a copy of this curious manuscript, made with scrupulous care, and this will be printed in a second volume soon after the first. We shall make further comment on these interesting volumes when they appear from the press.

VII. The Bahamas Hurricane of 1866.

The destructive hurricane which passed over the Bahamas in October, 1866, disabling many American steamers,* furnished a remarkable illustration of the theory of cyclones often

* Among them the "Evening Star" foundered.

expounded in this Journal by the late Mr. William C. Redfield. His son, Mr. John H. Redfield of Philadelphia, has drawn a diagram, illustrating the direction of the winds at noon on the first day of October, availing himself not only of the marine reports of our newspapers, but especially of the abstract of observations officially reported by His Exc. R. W. Rawson, Governor of the Bahamas. This diagram is so striking that we reproduce it with the following extract from Mr. Redfield's letter to Governor Rawson. It originally appeared in connection with the latter's Report, recently printed as a pamphlet at Nassau, N. P. The general course of the hurricane through the Atlantic is indicated on another map by Capt. W. H. Stuart. The cyclone was five days in progress from the Eastern Bahamas, to the latitude of Cape Hatteras.

Philadelphia, March 23, 1868.

* * * * It was this consideration that suggested to me to employ your abundant data for mapping out the actual winds of the storm, in the different portions of its area, at a definite period of time, and thus coördinating the facts of the case. This mode of analysis had been a favorite one with my father (Wm. C. Redfield, of New York), and was employed most elaborately, and with signal success in his Memoir upon the Cuba Hurricane of Oct., 1844. (See Am. Jour. Sci., II, vol. i, ii, Jan. to Nov., 1846.) Some embarrassment, which I experienced from an occasional want of more definite information from some of the points of observation as to the precise direction of the wind at the hour selected, has been removed by the kindness of Capt. W. H. Stuart, Deputy Inspector of Lighthouses, Bahamas, who, at your suggestion, has sent me all needed facts in the table subjoined. From this table the accompanying small chart is constructed, showing the directions of the wind at *noon* of Oct. 1, at 34 different points in various portions of the storm area. In regard to this chart, I have few annotations to make, preferring that it should tell its own story.

The center of the hurricane, at the hour named, was evidently but a few miles southeastward from Shroud Cay, the wind at that Cay being from N.E. until 1 P. M., when there was a lull of an hour, followed by the gale from S.W. At the points 4, 5, 7, 8, 9 and 10, which lie beyond, or just within, the retreating limb of the cyclone, we find, as might be expected, that the storm's violence was abating, while at points 31 to 35, just covered by the advancing limb, the tempest was increasing gradually as its vortex was approaching.

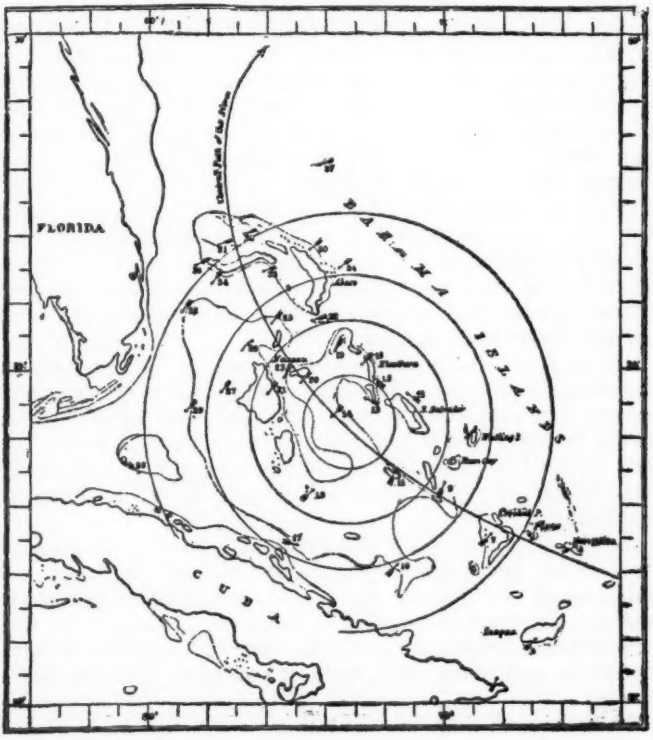
Observation No. 18 (Sch. *Violin*), seems somewhat anomalous in its character, and as no log was kept, and the crew could neither read nor write, it is not too much to suppose, either that they reported the wind too much to the S. of W., or else that the vessel's position was really farther to the eastward. [It is believed that the master and crew only guessed their position.—R. W. R.]

The line of progress of the axis of this storm, after leaving Turks Island, seems to have been almost identical with that of the Antigua Hurricane of Aug. 22, 1848, which also passed over the Bahamas, beginning its easterly recurvature also about latitude 28° or 29°, and pursuing its northeasterly track at least to long. 35° and lat. 45°, occupying twelve days in its journey.

The numbers upon the little chart refer to the places named in your Abstract of Observations, as well as to those of Capt. Stuart's table.

With great respect, I have the honor to remain,
Your obdt. servant, JOHN H. REDFIELD.

BAHAMAS HURRICANE OF OCTOBER, 1866,
Showing directions of wind at noon, Oct. 1.
FROM OBSERVATIONS COLLECTED BY GOV. RAWSON, W. RAWSON, C. B., AND CAPT W. H. STUART, BAHAMAS.
By JOHN H. REDFIELD, Philadelphia.



Art. X.—*Meteors of November 14th*, 1868.

On the morning of Saturday the 14th of November, 1868, there was another recurrence of the November star shower. The following are the accounts of the numbers and appearances of the meteors as reported from different places.

1. *At New Haven.*—The writer, with Mr. O. Harger, and several students in the College, watched from the top of the tower of Graduates Hall. We commenced counting as the clock struck twelve. There were at first seven or eight in the party, but within ten minutes this number increased to about twelve who were counting. Toward morning the number diminished to seven or eight. To each person was assigned a direction to which he was to look. From our station we had an unobstructed view of the heavens.

The counting was aloud to avoid duplication ; each meteor, whether seen by one or by several persons being counted once and once only. Such at least was our aim, and probably few were lost at first. But after a time as the meteors became more abundant it was evident that many were lost in the counting. For, when two or three came nearly or quite at the same instant, only one might be added to the number. We were then by no means certain that we were making a fair census. I directed each one, therefore, to count silently during intervals which I carefully limited by the watch. The numbers reported by each observer during these intervals were entered upon the record. Immediately upon the close of an interval the counting in concert was resumed.

In the first table given below there are in the first column the intervals of observation during which we were counting in concert ; in the second column the lengths of those intervals; in the third column the numbers actually counted in the interval; in the fourth the average number per minute; in the fifth the number of observers; and in the sixth the total number per hour.

In computing these *hourly* numbers, allowance is made for the omitted intervals by assuming for the rate per minute the half sum of the rates of the periods just preceding and following. In the four hours and forty-two minutes the numbers counted amounted to 5573. For the omitted intervals the above rule gives 1786, making a total of 7359 in the six hours.

In the second table are the numbers seen by each observer in the corresponding specified intervals. The average numbers per minute for each observer are given in the final column.

TABLE I.—*Showing the numbers of meteors seen at New Haven in successive intervals, on the morning of Nov. 14th, 1868, by the whole party observing.*

Interval of observation.		Duration.	Number.	No. per min.	No. of obs.	Hourly No.
From 12^h 0^m	to 12^h 15^m	15	141	9·6	10	
" 15	" 30	15	179	11·9	12	
" 30	" 40	10	134	13·4	12	
" 40	" 45	5	72	14·4	12	
" 45	" 50	5	87	17·4	12	
" 50	" 55	5	65	13·0	12	757
" 55	" 1^h 0^m	5	79	15·8	12	
" 1^h 0^m	" 5	5	79	15·8	12	
" 5	" 10	5	89	17·8	12	
" 10	" 15	5	71	14·2	12	
" 15	" 20	5	85	17·0	12	
" 20	" 25	5	83	16·6	12	
" 25	" 30	5	93	18·6	12	
" 30	" 35	5	84	16·8	12	
" 35	" 40	5	101	20·2	12	
" 40	" 45	5	116	23·2	12	
" 45	" 48	3	66	22·0	11	
" 50	" 55	5	95	19·0	11	
" 55	" 2^h 0^m	5	129	25·8	11	1132
" 2^h 0^m	" 2	2	39	19·5	11	
" 3	" 6	3	50	16·7	11	
" 6	" 10	4	101	25·2	11	
" 10	" 15	5	109	21·5	11	
" 15	" 20	5	111	22·2	11	
" 20	" 23	3	73	24·3	11	
" 25	" 30	5	114	22·8	11	
" 30	" 33	3	84	28·0	11	
" 35	" 40	5	118	23·6	11	
" 40	" 45	5	108	21·6	11	
" 45	" 52	7	149	21·3	11	
" 52	" 55	3	68	22·7	11	
" 55	" 3^h $0\frac{1}{2}^m$	$5\frac{1}{2}$	128	24·4	11	1369
" 3^h $0\frac{1}{2}^m$	" 3	$2\frac{1}{2}$	56	20·4	11	
" 5	" 11	6	115	19·2	11	
" 11	" 12	1	21	21·0	11	
" 13	" 18	5	110	22·0	11	
" 20	" 23	3	68	22·7	11	
" 23	" 28	5	103	20 6	11	
" 28	" 36	8	160	20·0	11	
" 38	" 41	3	78	26·0	8	
" 43	" $46\frac{1}{2}$	$3\frac{1}{2}$	102	29·1	8	
" 48	" $50\frac{1}{2}$	$2\frac{1}{2}$	59	23·6	8	
" 53	" $55\frac{1}{2}$	$2\frac{1}{2}$	49	19·6	8	
" 58	" 4^h $0\frac{1}{2}^m$	$2\frac{1}{2}$	59	23·6	8	1335
" 4^h 3^m	" 8	5	108	21·6	8	
" 8	" $10\frac{1}{2}$	$2\frac{1}{2}$	41	16·4	8	
" 13	" $15\frac{1}{2}$	$2\frac{1}{2}$	50	20·0	8	
" $20\frac{1}{2}$	" 24	$3\frac{1}{2}$	93	26·6	8	
" 28	" 33	5	102	20·4	7	
" 33	" 34	1	20	20·0	7	
" 38	" 43	5	84	16·8	8	
" $48\frac{1}{2}$	" 53	$4\frac{1}{2}$	131	29·1	8	1364
" 58	" 5^h 3^m	5	150	30·0	8	
" 5^h 7^m	" 13	6	193	32·2	8	
" 13	" 18	5	214	42·8	8	
" 23	" $25\frac{1}{2}$	$2\frac{1}{2}$	74	29·6	8	
" $30\frac{1}{2}$	" $33\frac{1}{2}$	3	61	20·3	8	
" 38	" 43	5	97	19·4	7	
" 46	" 48	2	27	13·5	7	
" 48	" $54\frac{1}{2}$	$6\frac{1}{2}$	48	7·4	7	1402

Total in 4^h 42^m, 5573; in six hours, 7359.

TABLE II.—*Showing the numbers of meteors seen in New Haven in the intervals of time named, on the morning of Nov. 14th, 1868, by single observers.*

Interval of observation.	Duration of interval.	Harger.	Dudley.	Lanman.	Williams.	Slocum.	Chapin.	Todd.	Perry.	Sweet.	Maynard.	Need.	Clark.	Av. no. per minute.
1h 48m to 1h 49m	1m	7	9		7	7	5	5	10		13	5	12	8.0
2h 2m " 2h 5m	1	2	1	2	0	5	6	5	6	6		4	7	4·0
23½ " 25	1½	11	13	10	13	10	13	14	16	11	16	16		7·8
33 " 35	2	19	21	14	10	14		12	24	9	23	18		8·2
3h 3m " 3 5m	2	11	11	12	6	12		15	13	15	12			5·9
12 " 13	1	9	6	8	7	4		4	8	0	4			5·6
18 " 20	2	12	21	20	19	24		19	22	21	24			10·1
36 " 38	2	14	20	14		17	7	18	19	14				7·7
41 " 43	2	22	25	19		22	32	34	23	11				11·7
46½ " 48	1½	12	8	10	14	13	17	12	18	5				8·1
50½ " 53	2½	17	13	17	22	18	23	17	20	18				7·3
55½ " 58	2½	20	22	16	28	16	16	27	21	18				8·2
4h 0½m " 4h 3m	2½	17	15	13	17	10	16	17	22	14				6·3
10½ " 13	2½	14	7	11	17	12	9	19		8				4·8
15½ " 20½	5	31		46	36	38	43	36	52	29				7·8
24 " 28	4	39	25	30	28	33	42	32	35	25				8·0
34 " 38	4	44	21		44	32	43	44	47					9·8
43 " 48½	5½	52	49	56	50	45	70	48	64					9·9
53 " 58	5	53	29	35	49	43	42	59	43					8·8
5h 3m " 5h 7m	4	45	40	35	44	48	49	54	34					10·9
18 " 23	5		30	37	54	58	66	60	83					11·1
25½ " 30½	5	38	38	46	47	48	78	52	65					10 3
33½ " 38	4½	31	9	35	36	23	15	48	27					6·2
43 " 46	3	19	12	4	6	5	12	18						3·6
54½ " 6h 0½m	6½	21	19	21	16	18	7	25						2·7

The short duration of the shower seems to imply that the radiant is very narrow perpendicularly to the ecliptic. Early in the morning hours it seemed that this area must extend parallel to the ecliptic nearly up to ε Leonis. But its length in that direction had then to be determined by flights that were nearly parallel to the horizon. The eye cannot easily make allowance for the curvature of the arc of a great circle in carrying backward the line of such a track. I feel sure that the tendency to make such tracks parallel to the horizon is so strong that in a careful location of the radiant, we must reject nearly all those in which the meteor first appears several degrees from the sickle. Yet after all allowances I believe that some paths in these early hours would when traced backward pass near ε Leonis. After the radiant had reached an altitude of 30 or 40 degrees, there were very few tracks, if any, which traced back would not cut across the line joining γ Leonis with the sixth magnitude star (230 Piazzi, 3423 B. A. C.) in the center of the bend of the sickle, and between these stars.

But there were many tracks which when extended backward cut this line at large angles and near either extremity of it. This implies that the radiant was not much shorter than the

distance between them. The latitude of the radiant I estimated as about that of γ Leonis, or $8\frac{1}{2}°$. To determine this latitude only meteors starting from near the radiant and moving nearly parallel to the ecliptic were noticed.

If the radiant was a point, any two well observed flights would determine its place. But in consequence of its considerable length, only flights nearly parallel to, or nearly perpendicular to, the ecliptic are convenient for determining its latitude, and its limits in longitude.

The prevailing tint of the trains was green, or bluish green. Mr. Harger counted 60 unconformable meteors during the six hours. These were strikingly unlike the conformable ones, giving usually the impression of a harder nucleus, and leaving no train.

Many trains were visible for several minutes, and one remained for 44 minutes. They usually floated to the northward.

The sky was beautifully clear, and moonless. There was no abatement in the numbers of the meteors until dawn. Throughout the shower the proportion of faint meteors was very small. As a consequence, the most fruitful regions of the sky were nearer the horizon than in August showers. During the forenoon of the 14th, I watched clear portions of the sky for a short time, but saw no indications of the meteors.

2. *At Bowdoin College, Brunswick, Me.*, communicated by Prof. C. G. Rockwood, Jr.—In the count of meteors, I was assisted by Mr. J. P. Gross and a number of students.

The number varied from thirteen to sixteen, being at no time less than thirteen, nor more than sixteen; the place selected was on the College grounds, where we had a view of the sky on all sides nearly down to the horizon. The night was perfectly clear, the sky being free from clouds.

The display began before midnight, but the party was arranged and the formal count began at 51^m after midnight.

After 1 o'clock the numbers were recorded (by myself) at intervals of five minutes, the numbers seen being given below:—

	1h to 2h		2h to 3h		3h to 4h		4h to 5h		5h to 6h	
m m	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.
0— 5	67	1	91	4	103	0	123	2	149	0
5—10	68	2	89	4	109	2	132	2	165	4
10—15	75	3	86	3	105	0	110	2	150	0
15—20	90	8	108	3	99	0	124	1		
20—25	84	4	104	4	106	1	131	1		
25—30	91	5	107	3	104	0	135	1		
30—35	75	2	108	2	108	4	134	0		
35—40	91	0	84	2	118	3	126	1		
40—45	90	2	96	3	107	6	123	0		
45—50	92	7	95	4	140	3	115	0		
50—55	110	5	82	0	123	3	142	0		
50—60	107	6	114	2	111	3	150	2		
Total,	1040	45	1164	34	1336	25	1545	12	464	4

The whole number counted in $4^h\ 24^m$ was 5670, of which number 121 were reported as not conformable to the radiant in Leo. But as the observers were all without previous experience in such work but little reliance can be placed on the number of unconformable meteors recorded.

Almost all the brighter meteors left trains of various lengths, and frequently several trains would be visible upon the sky at one time. These presented the usual appearance of clouds of luminous smoke, which gradually changed their form, and floated toward the north. Many meteors were so brilliant as to cast a light on surrounding objects, which was plainly visible even when the meteor itself was hidden from view by some intervening object. Very many were brighter than Venus, which was visible in the east during the latter part of the watch.

3. *At Boston, Mass.*—Mr. T. W. Tuttle, from a window looking north, saw as follows:

From	3^h	7^m	to	3^h	22^m	92 meteors.	From	5^h	0^m	to 5^h	4^m	20 meteors.
"		22	"		43	52 "	"		4	"	8	17 "
"		43	"		55	34 "	"		8	"	14	29 "
"		55	"	4^h	4	26 "	"		14	"	20	31 "
"	4^h	33	"		43	47 "	"		20	"	27	25 "
"		43	"		46	20 "	"		27	"	30	12 "
"		46	"		54	20 "	"		30	"	38	10 "
"		55	"	5^h	0	20 "		Total in $2^h\ 2^m$ 455				

4. *At New Bedford, Mass.*—Mr. R. Taber reports the following numbers for three observers:

From	10^h	0^m	to	10^h	30^m	19 meteors.	From	1^h	30^m	to	2^h	0^m	237 meteors.
"	11	0	"	11	45	52 "	"	2	0	"	2	30	101 "
"	11	45	"	12	0	53 "	"	2	20	"	2	0	212 "
"	12	25	"	1	0	170 "	"	3	0	"	2	30	271 "
"	1	0	"	1	30	215 "							

For one observer:

From	3^h	30^m	to	4^h	0^m	190 meteors.	From	4^h	30^m	to 5^h	155 meteors.	
"	4	0	"	4	30	159 "	"	5	0	" 6	207 "	

The flights were in general unusually bright, leaving in many cases long trails, which remained visible sometimes two, and four, and in one instance eight minutes; gathering apparently in knots and waves, with an apparent motion of its parts and curving like a trail of smoke in a light wind. One of these examined by the telescope, showed plainly a difference in the intensity of its light, being much more luminous on the knots, so to speak; yet not enough to dim the intensity of the third magnitude star which shone through it.

5. *At Williamstown, Mass.*—Messrs. Benjamin J. Gilman, W. D. Granger and F. B. Wilder, saw

between	11^h	35^m	and	12^h 35^m	450 meteors.
"	12	35	"	1 20	250 "
"	3	0	"	4 0	950 "
"	4	0	"	5 0	830 "

Total in $3^h\ 45^m$ 2480 by three observers.

6. *At Stamford, Conn.*—Mr. E. A. Fuertes was occupied principally in locating the tracks of the meteors upon the chart. He estimates the number seen by him at more than a thousand. The sky was magnificent, the belts of Jupiter appearing with a brownish red color which he has only seen in remarkably fine nights with his glass, of six feet focus and $4\frac{1}{2}$ inches aperture. He thinks that in the earlier part of the evening the meteors were green, and gradually changed to blue as the night advanced.

7. *At Poughkeepsie, N. Y.*—Miss Mitchell reports from five observers 3,766 meteors. The hour most fruitful was that from two to three o'clock, in which 900 were counted. The most fruitful minute was from $2^h\ 24^m$ to $2^h\ 25^m$. The other hours from 12^h to $5\frac{1}{2}^h$ were much alike. Flashes of light for which they could not account by any meteor above the horizon were frequent, and Miss Mitchell was confident that the evening of Nov. 13th was lighter than common for a moonless night without aurora.

8. *At Palisades, N. Y.*—Mr. W. S. Gilman, Jr., gives the following summary of the numbers of meteors seen by himself and Mr. Thomas P. Gilman. The latter was constantly counting, but the former was occupied principally in noticing the peculiarities of the more remarkable ones, and mapping the paths upon the chart. He judged that the numbers were 25 or 30 per cent of the true number visible. A few meteors of great beauty were seen before $11\frac{1}{2}$ o'clock.

Time.	Number.	Barometer.	Therm.	Sky.	Wind.
$11\frac{1}{2}^h$ to 12^h	32	30·105	37°	Clear.	W.
12 " 1	182	·110	38	"	Light.
1 " 2	294	·102	38	"	Very still.
2 " 3	304	·100	32	"	Still.
3 " 4	290	·095	31	"	Still.
4 " 5	238	·080	32	Slight haze in Leo.	Fresher; west.
5^h " $5\frac{1}{2}$	122	·077	30	"	Light N.W. wind.

"I could distinctly, and with the utmost ease, distinguish the little companion of Sirius in my 4-inch glass with a power of only 40, at 3^h A. M., which shows remarkable clearness and steadiness of atmosphere. I noticed blue trains at first, afterward more greenish ones. The radiant point seemed to me to be near η Leonis this year, say R. A. 152°, N. Dec. 18°."

Mr. Gilman has furnished many valuable observations upon individual meteors and trains, which will be of special value when compared with those made at other places upon the same bodies.

9. *At Philadelpha.*—Mr. Pliny E. Chase counted from a window, between 1^h and 2^h A M., 155 meteors; between 2^h and 3^h, 163; between 3^h and 4^h, 206; between 4^h and 5^h, 221; and between 5^h and $5^h\ 28^m$, 105. The successive hundreds

were counted in 35, 47, 32, 22, 39, 27, 25, and 26 minutes, and the remaining fifty in 15 minutes. Among the number seen were 15 couplets and 1 triplet. Only 25, or 3 per cent, were nonconformable.

10. *At Haverford, Pa.*—Prof. S. J. Gummere furnishes a report of observations made at Haverford College under his direction, though he could himself watch only from a window. The report is by Mr. E. B. Taylor.

Counting was begun at $11^h\ 34^m$, and the time of finishing the even hundreds, and the numbers per minute, are as below:

Time.	Entire no.	No. pr m.	Time.	Entire no.	No. pr m.	Time.	Entire no.	No. pr m.
12h 4m 0s	200	6·7	2h 39m 26s	1900	12·1	4h 38m 14s	3500	12·1
34 0	400	6·7	46 40	2000	13·8	43 0	3700	41·9
46 3	500	8·3	53 3	2100	15·7	46 38	3800	27·5
58 18	600	8·2	3h 9 0	2300	12·5	50 31	3900	25·7
1h 17 0	800	10·7	17 34	2400	11·7	53 38	4000	32·1
34 13	1000	11·6	30 55	2600	15·0	55 27	4100	55·0
41 55	1100	13·0	45 26	2800	13·8	59 26	4200	25·1
47 13	1200	18·9	51 23	2900	16.8	5h 25 33	4700	19·1
55 25	1300	12·2	58 13	3000	14·6	30 55	4800	18·6
2h 1 25	1400	16·7	4h 7 2	3100	11·3	35 45	4900	20·7
8 57	1500	13·3	14 29	3200	13·5	40 17	5000	22·1
23 25	1700	13·8	22 1	3300	13·3			
31 12	1800	12·8	29 58	3400	12·6			

Several hundred more were counted, but the times were not observed.

11. *At Magnetic Observatory, Toronto, Canada.*—Communicated by the director, Mr. G. T. Kingston. Nearly 3000 were counted between $10^h\ 45^m$ and $18^h\ 0^m$ of Nov. 13th, Toronto astronomical time. With the exception of about one per cent their courses were from the constellation Leo.

Owing partly to the remarkably favorable state of the sky during most of the night many of the meteors appeared very large and brilliant, some exceeding Sirius in apparent magnitude, and often exhibiting a variety of colors. Most of them were followed by trains which often left tracks that continued visible from two to four minutes. Two observers were constantly watching excepting from $10^h\ 45^m$ to $11^h\ 0^m$ and from $11^h\ 50^m$ to $12^h\ 10^m$, when only one was engaged. From $12^h\ 10^m$ to $17^h\ 0^m$ a third observer was frequently though not constantly assisting.

The annexed table shows the number seen at different points of the night together with the corresponding state of the sky.

	Time. h m — h m	Number counted.	State of the sky.
Nov. 13.	10 45 — 12 0	173	Very clear.
	12 0 — 13 0	329	"
	13 0 — 14 0	583	"
	14 0 — 15 0	489	Occasional very light haze.
	15 0 — 16 0	375	" "
	16 0 — 17 0	572	Haze increasing.
	17 0 — 18 0	365	Clouds 0·4, and very hazy.
	Total,	2886	

12. *At Marathon, N. Y.*—Mr. Lewis Swift, after half past one o'clock, counted 896 meteors, all but five from Leo. At a little after three o'clock a train in Cancer was visible for several minutes which floated to the *north*.

13. *At Bloomington, Ind.*—Prof. T. A. Wylie reports observations by some of the students of the Ind. State University. He had made no arrangements himself for observing as he had watched during the whole of the preceding night, judging, with good reason, that there was more probability of a display on the morning of the 13th.

The whole number observed by the students was 2,500. The numbers increased gradually from 11 o'clock to 3 o'clock, when it reached 20 per minute, at which rate it continued until a quarter before four. The sky then became hazy, and by $4^h\ 40^m$ quite cloudy. At four minutes past five they began regular counting with these results.

From	$5^h\ 4^m$	to	$5^h\ 7^m$	50	meteors.	
"	7	"	10	50	"	
"	10	"	15	56	"	
"	15	"	18	50	"	
"	18	"	22	50	"	
"	22	"	27	50	"	
"	27	"	30	55	"	Clear.
"	30	"	35	50	"	
"	35	"	39	50	"	
"	39	"	43	50	"	
"	43	"	47	59	"	Hazy.
"	47	"	51	50	"	
"	51	"	59	50	"	Cloudy near horizon.
"	59	"	$6^h\ 11$	50	"	

The train of one meteor at $5^h\ 25^m$ remained ten minutes in sight, moving slowly *eastward*. The meteor passed through the bowl of the great dipper.

Prof. Kirkwood and Mr. Maxwell thought they saw one meteor in the forenoon of the 13th, and suspected two or three others. They were looking from a shaded place, in the direction of the radiant. Prof. Kirkwood, however, in view of the fact that no considerable fall seems to have commenced until several hours later, thinks it quite probable that there was some optical deception.

14. *At Vevay, Ind.*—Mr. Charles G. Boerner reports the following numbers seen by himself and two assistants in successive intervals, beginning at $1^h\ 4^m$ and ending at $5^h\ 37^m$:

From 1h 4m to 3h.			From 3h to 4h 37 m.			From 4h 37m to 5 37m.		
In 30^m	17	meteors.	In 15^m	138	meteors.	In 13^m	161	meteors.
30	90	"	15	114	"	10	156	"
11	48	"	15	162	"	16	147	"
15	116	"	15	119	"	15	102	"
15	93	"	18	111	"	6	25	"
15	153	"	19	174	"	—	—	
						Total in $4^h\ 37^m$,	1926	"

15. *At Des Moines, Iowa.*—Mr. J. E. Hendricks counted in an hour and a quarter, from four o'clock, 250 meteors, only one being nonconformable.

16. *At Manhattan, Kansas.*—Prof. B. F. Mudge with one assistant counted 833 from half past four to half past five o'clock. The time per hundred varied from seven to nine minutes, the last hundred being counted in eight minutes.

As Prof. Mudge was $1^h\ 35^m$ west of New Haven (long. 96° 40′), and as this number for two observers corresponds to about 1,700 for eight observers, his numbers indicate that there was no diminution, but rather an increase in the intensity of the display just after dawn at New Haven.

17. The above abstract of observations is confined principally to the numbers seen on the morning of Nov. 14th. The most palpable peculiarities of the display are its uniform intensity through several hours, and its appearance twelve or eighteen hours later than might have been expected.

We are indebted to Commodore Sands, Prof. Rockwood, Mr. W. S. Gilman, Miss Mitchell, Mr. Fuertes, Mr. Tuttle, Prof. Gummere, Mr. Swift, Mr. Wm. C. Taylor of Philadelphia, Prof. Twining, and others, for valuable observations upon particular meteors remarkable for brilliancy or duration of train. These, together with a few observations on other nights, and some general considerations respecting the display, will furnish matter for a continuation of this article in another number of the Journal.

Meanwhile we respectfully request any persons within two hundred miles of New Haven, who may have observations upon particular meteors, to communicate them to us, or exchange them with ours.

Especially desirable would be any additional observations upon the remarkable meteor which passed about 80 miles north of Philadelphia, and disappeared at an altitude of about 50 miles at a point over Schuylkill Co., in Pennsylvania, at $1^h\ 16^m$, New Haven time. The portions of its train floated in different directions, and continued visible nearly or quite three-fourths of an hour. It was seen and its place noticed, at Williamstown, New Haven, Poughkeepsie, Palisades, Haverford, and other places.

For several communications we are indebted to the courtesy of Prof. Henry, Secretary of the Smithsonian Institution.

H. A. N.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *Spectral analysis of the protuberances observed during a total eclipse of the sun.*—The total eclipse of the 18th of August, 1868, attracted an even more than usual degree of attention. It was observed at Wah-Tonne on the peninsula of Malacca by Rayet; at Guntoor by Janssen; at Beejapoor by Capt. Haig, R. E.; at Barram Point on the Island of Borneo by Hennessy; and at Jamkhandi by Lieuts. Herschel and Campbell. The observations made by means of the spectroscope were particularly interesting and important, and agree upon the whole remarkably well. Rayet was provided with a silvered glass reflecting telescope, the reflector twenty centimeters in diameter, and a direct vision spectroscope with three prisms. When the image of a long protuberance on the eastern border of the sun was brought upon the slit of the spectroscope, a spectrum was seen with nine brilliant lines; of these, four corresponded respectively to B, D, E and F; two corresponded to lines in *b*; two to lines in the group G, and there was one unknown line. The western portion of the sun's margin gave the same lines except that only one violet line was visible. With respect to these observations we may remark that according to the other observers Rayet must have mistaken C for B, and also that the line which he identifies with D is really more refrangible, being separated from it by an interval too large to admit of a doubt as to the want of coincidence. In his letter to the Académie des Sciences Janssen gives no details of the spectral lines observed by him, but communicates the important result that the spectra of the protuberances may be seen at all times, so that it is not necessary to wait for an eclipse. His conclusion is that the protuberances are immense gaseous masses, chiefly composed of hydrogen. Though the merit of having first observed this most important fact belongs undoubtedly to Janssen, it appears that in 1866 Mr. Norman Lockyer suggested that the spectra of the protuberances might be seen by examining the margin of the sun's disc, but did not succeed in observing these spectra until the 20th of October of the present year, or two months after the observation had been made by Janssen. On the 20th of October Mr. Lockyer observed the spectrum of a protuberance, and found it to consist of three lines,—one exactly corresponding with C, one very nearly with F, and one eight or nine degrees of Kirchhoff's scale more refrangible than D. There was no ray at B or at *b;* the region near G was not examined. Observations recently made in Cambridge by Prof. Winlock with the 15-inch equatorial and with a spectroscope with two flint glass prisms of 60°, give the same results. Mr. Lockyer also found that the three bright rays extended to a small distance upon the surface of the sun, and that their lengths were different, the red being the shortest. In a very

interesting and suggestive paper "On the physical constitution of the Sun and Stars," communicated to the Royal Society, May 15th, 1867, Mr. G. J. Stoney remarks that he has several times thought that he observed a bright line somewhat more refrangible than D in examining the sun with a spectroscope composed of two flint glass prisms, as well as two other lines, one between 1025·5 and 1027·7 of Kirchhoff's scale, and another between the two lines D. During the summer of the present year Mr. Stoney saw several faint bright lines in other parts of the spectrum, one coinciding with or very near Kirchhoff's copper line of wave length 522·3. There can be no doubt that the line seen near D was one of those noted by Mr. Lockyer.—*Rayet in Comptes Rendus*, lxvii, 759; *Janssen in Comptes Rendus*, lxvii, 838; *Lockyer in Comptes Rendus*, lxvii, 904; *Stoney in Proceedings of Royal Society*, xvii, p. 1. W. G.

2. *On the magnetism of chemical combinations.*—Wiedemann, to whom we owe most of our precise knowledge of the magnetism of compounds, has published a new series of observations of much interest. These all confirm the law already announced by the author that in salts of similar constitution containing the same metal, the magnetism referred to the equivalent of the compound, or the atomic magnetism, is sensibly constant. The author's new results may be stated as follows:

(1.) Observations made by the method used in his former investigations show that the same relations exist for the haloid and oxy-salts of cerium, didymium and oxyd of copper (CuO). Taking the unit previously established, we have for the atomic magnetism of aqueous solutions of the salts above mentioned the following numbers:

Sulphate of didymium,	104·4
Nitrate "	104·2
Chlorid "	105·2
Acetate "	105·7
Nitrate of cerium (CeO),	48·7
Chlorid of cerium (CeCl),	47·6
Sulphate of copper,	49·5
Nitrate of copper,	50·7
Chlorid of copper, (CuCl),	48·9
Bromid of copper, (CuBr),	47·7
Acetate "	48·0

(2.) We obtain approximately the same values for the solid salts, especially when they contain water of crystallization. For anhydrous salts the atomic magnetism is in general a little less than for the hydrated salts. This diminution is particularly sensible for the salts of copper and nickel.

(3.) Experiments prove that two diamagnetic elements—as for example, copper and bromine—may give a magnetic compound.

(4.) The author has examined the magnetism after double decomposition of two saline solutions of magnetism M_1 and M_2 known

before mixture. After reaction the magnetism is sensibly equal to the sum M_1+M_2. In the greater number of cases the formation of a precipitate exerts no disturbing influence.

(5.) The atomic magnetism of hydrated oxyds is a little higher or lower than that of the corresponding salts.

(6.) The magnetism of precipitated hydrate of sesquioxyd of iron increases rapidly, beginning at the moment of precipitation. The author attributes this fact to the colloidal state of the hydrate at the first moment of precipitation. The differences presented by the atomic magnetism of acetate of sesquioxyd of iron doubtless arise from the same cause. These differences are not remarked in the solution of hydrate of sesquioxyd of chromium, in caustic potash, and in that of oxyd of nickel in ammonia.

(7.) The magnetism of the anhydrous oxyds of the magnetic metals is notably less than that of the saline combinations of these oxyds.

(8.) With the exception of sulphate of manganese, the magnetism of the sulphates of the magnetic metals is very feeble.

(9.) The magnetism of the cyanids of nickel and cobalt disappears almost completely when the cyanids are dissolved in cyanid of potassium, which would not happen if this solution be considered as a double salt.—*Comptes Rendus*, lxvii, 833, *from Monatsbericht der Königl. Preuss. Akad. for July*, 1868. W. G.

3. *On a new series of chemical reactions produced by light.*—Tyndall has communicated to the Royal Society the results of experiments made by subjecting the vapors of volatile liquids to the action of concentrated solar or electric light. The author's paper is too full of detail to admit of an abstract, but we shall give a few of the more salient results. A tube 2·8 feet long and 2·5 inches internal diameter is closed at both ends by glass plates. It may be connected with an air pump and with a series of tubes used for the purification of air. A number of test tubes were converted into Wolfe's bottles by means of corks and tubes. Each test tube was filled partly with the liquid to be examined and introduced into the path of the purified air. When the experimental tube was exhausted and the air then allowed to bubble through the liquid, a mixture of air and vapor entered the experimental tube together, and were then submitted to the action of light. At one end of the experimental tube was placed an electric lamp transmitting an intense beam of light through the tube parallel to its axis. When the vapor of amylic nitrite was allowed to enter the tube in the dark and the beam of light was then sent through the tube, the tube appeared for an instant optically empty; then a sudden shower of liquid spherules was precipitated on the beam. On repeating this experiment with a condensed beam of light forming a cone eight inches long, the cone which was at first invisible flashed suddenly like a luminous spear. The rapidity of the condensing action diminished with the density of the light. The same effects were produced when oxygen or hydrogen were

employed as carriers; when the head of the beam was sifted out through a plate of alum, or when the beam was used without sifting. That the amylic nitrite undergoes decomposition is proved by the formation of brown fumes of nitrous acid. Sunlight produces similar effects. The author proves in the next place that the decomposition is effected by the more refrangible rays of light, and that liquid amylic nitrite is most potent in arresting the rays which affect its vapor. This seems to show that the absorption takes place in the atoms and not in the molecules. The author anticipates wide, if not entire, generality for the fact that a liquid and its vapor absorb the same rays. When the tube is filled with a rare and well mixed vapor, the electric light develops a blue color, which may be pure and deep or milky according to the intensity of the light. The author connects this result with those of Brücke on the colors of the sky. Various other liquids were tried with success. In many cases the condensed vapors formed extremely beautiful and regularly shaped clouds, the particles rotating around the axis of the tube, or round other axes. The most beautiful forms appear to have been those produced by iodhydric acid.—*Proc. of the Royal Society*, xvii, p. 92. W. G.

4. *On the carbonaceous matter of meteorites.*—The analyses of Wöhler and Cloez have shown that certain meteorites contain substances composed of carbon, oxygen and hydrogen, and resembling the last residues of organic substances of terrestrial origin. Berthelot has applied to the carbonaceous matter of the meteorite of Orgueil the method of hydrogenation employed by him with so much success, and has succeeded in producing a notable quantity of hydrocarbons of the formenic series ($C_{2n}H_{2n+2}$) comparable with the oils of petroleum. This result forms a new analogy between the carbonaceous matter of meteorites and substances of organic origin found upon the surface of the earth.—*Comptes Rendus*, lxvii, 849. W. G.

Prof. Brush has called my attention to Mr. Henry's paper, with which I was not acquainted at the time of the publication of my own process for the estimation of manganese as pyrophosphate. That full justice may be done, Mr. Henry's paper is by my request republished in full.—W. Gibbs.

5. *On the separation of Nickel and Cobalt from Manganese*; by T. H. Henry, Esq., F.R.S.—The methods given by the best authorities for the separation of nickel and cobalt from manganese are either inconvenient or inaccurate. The only method which affords exact results is that of Ebelmen, in which the sulphids formed at a high temperature are acted upon by dilute hydrochloric acid; but it is both tedious and unpleasant to pass sulphuretted hydrogen over the oxyds contained in a porcelain tray in a porcelain tube at a red heat; and the modification suggested by Wöhler, of converting the oxyds into sulphids by fusing them with sulphur and carbonate of soda, still furnishes the sulphid of nickel in such state that it is slightly acted upon by very dilute hydrochloric acid.

I have obtained very accurate results by a process differing altogether from those described, but as simple as any.

When chlorid of ammonium and ammonia are added to a warm solution of sulphate of manganese or chlorid of manganese, and afterward phosphoric acid till the precipitation ceases, the whole of the manganese is precipitated; and after filtration no precipitate or even cloudiness is produced by the addition of sulphid of ammonium to the solution. The salt formed is, according to Otto, NH_4O 2MnO, PO_5+2HO, and after ignition 2MnO+PO_5. When a solution of chlorid or sulphate of nickel is treated in a similar manner, no precipitate occurs, even on standing a few days in a vessel lightly covered, when sufficient chlorid of ammonium and free ammonia are present.

The following example will show the mode by which I operate.

12·6 grs. of pure sulphate of manganese, $MnOSO_3$+4HO, were gently ignited, and weighed 8·73 grs. =4·11 MnO. 6 grs. of oxyd of nickel, containing a trace of silica, were ignited and weighed then 5·63 grs.; these were dissolved together in hydrochloric acid and water, and the solution diluted; it was made acid with hydrochloric acid, phosphoric acid was added, and the whole heated until nearly boiling: when ammonia was added in excess, a white bulky precipitate was produced which rapidly contracted in volume and became crystalline; after standing twelve hours it was filtered and washed with a solution of chlorid of ammonium and ammonia (the chlorid of ammonium is absolutely necessary). The precipitate was perfectly white, and remained so after ignition, when it weighed 8·67 grs. = 4.33 MnO; it was decomposed and examined, it did not contain a trace of nickel.

The ammoniacal solution was treated by sulphuretted hydrogen, and the sulphid converted into oxyd by roasting with a little carbonate of ammonia. It weighed 5·57 grs.

	Taken.	Obtained.
MnO - - - -	4·11	4·33
NiO - - - -	5·63	5·57

I probably drove off a little sulphuric acid on igniting the sulphate of manganese. I have obtained more accurate results with the manganese since.

In operating with cobalt and manganese in the same manner, I obtained a slight excess of manganese, and the salt was slightly pink, but on repeating the operation the separation was complete. I took 3·22 grs. of oxyd of manganese, and obtained 3·15 grs. free from cobalt.—*Phil. Mag.*, IV, xvi, 197.

II. MINERALOGY AND GEOLOGY.

1. *Note on the supposed Fossil Footmarks in Kansas;* by C. H. HITCHCOCK. (Communicated for this Journal.)—In this Journal, II, vol. xli, p. 174, is a description by Prof. B. F. Mudge of Kansas, of a slab containing four impressions referred to *Leptodactylous* birds. The rock was conjectured to belong to the Lias, and the specimen was obtained from the top of a bluff one hundred and twenty-five feet above the level of Republican river, in the central part of the State. The slab was much weathered, and careful search brought no other instance of the impressions to light.

It was my fortune to examine this specimen at Manhattan the past summer, but during the absence of Prof. Mudge. It had been suggested by eminent authority that these impressions were probably the marks of exogenous leaves. Little credence was given to this view, inasmuch as Prof. Mudge had favored us years before with a careful drawing of the slab; yet we were not prepared for the disappointment produced by the first glance at the slab. No two of the impressions were alike, and they had been obviously chipped out of the stone by the aborigines.

By reference to the description, it may be said that impression A is very much narrower than B, and less skillfully excavated. The edges are rough. Between C and D there is great dissimilarity. The middle toe of the first is very much longer than either of what might be called the middle toe of D, the latter having four toes. The divarication of the toes in A does not correspond with that in B, nor that of C with D, as ought to be the case if these footmarks had been made by the stepping of an animal.

The physical character of the impressions shows them to be unlike ichnites; for the excavation has a sharp edge, while the surface slopes gradually from the top of the layer to the bottom of the furrows in all genuine footmarks.

It is not the first time that aboriginal markings have been mistaken for animal remains. In the Hitchcock Ichnological Museum, Case No. 31, are numerous examples from Westmoreland county, Pennsylvania, of the same character. The roughness of the marks is more obvious than in the Kansas specimens, while the imitations of the tracks of dogs, birds, and other animals had received long generic and specific names. (See this Journal, II, vol. i, p. 268, 1846.) I have also heard of several cases not described in any scientific communication, but mentioned by friends. Hence, it appears to have been a habit of the aborigines to engrave upon the rocks imitations of the feet of animals, whether undesignedly or not does not appear.

The rock resembles some portions of the Connecticut River Sandstone, and it may be that genuine impressions will yet be discovered in Kansas. Prof. Mudge has manifested great enthusiasm in collecting specimens in the State of his adoption, and we anticipate the appearance of valuable papers from his pen.

In this connection it may be worthy of remembrance that in 1866 I found a single impression of the *Cheirotherium* on the Delaware river in Pennsylvania, a few miles below Easton, and that in the following year I found on the New Jersey side an impression of the *Grallator gracilis*, and one or two small reptilian ichnites. These are all Triassic.

26 Pine st., New York, Nov. 2d, 1868.

2. *On Ivigtite;* by G. HAGEMANN.—Over a year ago I made some examinations of a mineral occurring in the cryolite of Greenland; but as my analyses were too imperfect to determine therefrom a new mineral species, I did not publish them. Mr. Theo. D. Rand has, however, recently published a still more incomplete analysis (see this Journal, II, xlvi, November, 1868) of the same mineral, which he has named *ivigtite*, and I think it therefore proper also to make my examinations known, as they may throw some more light on this species.

Besides the occurrence already mentioned by Mr. Rand, ivigtite is found in small veins passing through the cryolite without reference to its cleavage. It has then a gold-yellow or pale-green color, and forms radiated plates, which seem as elastic as mica. I have also found it forming single stars of radiated plates in the solid cryolite. Sometimes the veins thicken, and the ivigtite appears then as an olive-green solid mass, often quite transparent.

It does not fuse before the blowpipe (Mr. Rand has probably got his sample mixed with cryolite, as he has found it to melt readily). It dissolves in borax with an iron and silica reaction. When heated in a closed tube it affords water with an acid reaction. Analyses I. and III. were made by fusion with carbonate of soda; II. was decomposed with fluohydric acid.

	I (yellow crystals).	II.	III (green mass).	Mean.
Silica,	40·00	----	42·82	41·41
Sesquioxyd of iron,	37·92	39·02	13·06	39·01
Alumina,			27·03	
Soda,	----	10·27	----	10·27
Potash,	----	1·05	----	1·05
Water,	3·06	----	3·93	3·49
Fluorine,	trace	----	trace	trace
Loss,	----	----	----	4·77
				100·00

Adding the alkalies to analysis III, we have

$\dot{S}i$ 42·82, $\dot{F}e$ 13·06, $\dot{A}l$ 27·03, $\dot{N}a$ 10·27, $\dot{K}$ 1·05, $\dot{H}$ 3·93, F *tr.* = 98·16.

This would give nearly the ratio of a unisilicate, 1 : 6 : 8 : 1, and the formula:

$$3(\dot{A}l_2, \dot{F}e_2)\ddot{S}i_2 + (\dot{N}a, \dot{K})\ddot{S}i + H\ddot{O}.$$

The great difference between Mr. Rand's and my analyses can perhaps be accounted for by Mr. Rand's having had a considerable quantity of cryolite in his sample. The cryolite would increase

the soda and decrease the alumina and silica. This would perhaps account also by its fluorine for a loss of 11·68 per cent in Mr. Rand's analysis. Of my samples, I. was not entirely free from cryolite; II. was more so, and III. I should judge almost entirely.

3. *Notes on the Chemical Geology of the Gold-fields of California;* by J. ARTHUR PHILLIPS. Communicated by Prof. A. C. RAMSAY. (Proceedings Royal Soc., xvi, 294.) (Abstract.)

Rocks of the Gold-Regions of California.—The great sedimentary metallic belt of California lies on the western slope of the Sierra Nevada, beginning in the neighborhood of Tejou Pass, and extending through the state to its northern limit. In consequence, however, of various local circumstances, different portions of this band are of very unequal importance as gold-producing districts.

The slates of the auriferous belt have been shown by Professor Whitney to belong, for a great extent, to the Jurassic period, although the occurrence of numerous Triassic fossils in the gold-bearing rocks of Plumas county and elsewhere renders it more than probable that no inconsiderable portion of the slates in the heart of the gold region are of that age.

The rock constituting the principal mass of the Sierra Nevada is a granite containing only a small proportion of quartz, and in which but one species of feldspar (oligoclase) is generally found.

Lying between the band of metamorphic slates and the great central mass of granite forming the more elevated portions of the chain, are found various crystalline rocks, such as syenites, diorites, and porphyries.

Quartz Veins.—The matrix or gangue of the auriferous veins of California is invariably quartz, which is generally crystalline in its structure, or partially vitreous and semi-transparent. In the majority of cases the quartz constituting an auriferous veinstone is ribboned in such a way as to form a succession of layers parallel with the walls of the lode itself; and some one or more of these laminæ are not unfrequently far more productive than all the others. In some instances these parallel bands are separated from each other by a thin layer of quartz, slightly differing, either in color or structure, from that forming the seams themselves; or they may be only distinguished by a difference of color of two adjoining members of the series.

In many cases, however, laminæ of the enclosing slates divide the vein into distinct bands; and in such instances it will be observed that the thickness of the interposed fragments of slate is sometimes not greater than that of a sheet of the thinnest paper. Cavities or druses containing crystals of quartz occur in all the auriferous veins of the country; and a certain amount of crystallization may also not unfrequently be remarked along the lines of junction of the several bands of which a vein is composed. In addition to ordinary quartz, in a more or less crystalline form, amorphous hydrated silica, or semiopal, and chalcedony are occa-

sionally met with; in some instances the opal is interfoliated between layers of true quartz, and is sufficiently auriferous to repay the expenses of treatment.

The metallic minerals enclosed in the gangue of auriferous veins are ordinary iron pyrites, blende, and galena, and less frequently, arsenical pyrites, magnetic and copper pyrites, and cinnabar. These sulphids invariably contain gold; and veins in which some one or more of them does not occur in considerable amounts, are not regularly and lastingly productive.

Near the surface the iron pyrites and other sulphids become decomposed by the action of air and the percolation of meteoric water through the mass, staining the quartz of a red or brown color, and leaving the gold in a free state. Under such circumstances numerous cubical moulds of iron pyrites are found in the veinstone; and although this mineral has been entirely removed by chemical action, the cavities left contain finely divided gold, obviously liberated by the decomposition of pyrites.

Beneath the line of natural drainage of the country the sulphids remain undecomposed; but if rock containing pyrites be placed in nitric acid the sulphid becomes dissolved, and finely divided, crystalline, or filiform gold will partially occupy the resulting cavities.

In one of the detrital beds in the vicinity of the village of Volcano in the county of Amador, and elsewhere, distinctly marked quartz veins may be observed cutting through the gravel, and evidently formed by the action of water holding silica in solution.

Attention has also been recently directed to bands of auriferous slate found in the copper-bearing belt west of the main gold-belt of the State, and in the foot hills of the Sierra. In this locality the gold, instead of being obtained from a well defined vein, chiefly composed of ordinary quartz, is inclosed in a band of siliceous slaty rock, extending northwest and southeast, and dipping in conformity with the other strata of the district.

The number of fluid-cavities contained in the veinstones of the auriferous lodes of California is seen under the microscope to be exceedingly limited; and in order to obtain sections affording good examples, even of small size, it is necessary to select such bands as may be more than ordinarily crystalline, or to operate on thin fragments of crystals sometimes found lining the interior of drusy cavities. In the more opaque and generally most auriferous portions of veins, the cavities are numerous but exceedingly small, and are often so opaque, apparently rendered so by being internally coated with a lining of clay, that no vacuities can be distinguished.

Out of more than fifty sections of veinstone examined, only some six or seven were found to contain fluid-cavities of sufficient size to admit of any attempt at accurate measurement by means of ordinary appliances; but in all cases there appeared to be considerable differences in the relative dimensions of the vacuities and

the enclosing cavities, and the temperatures at which they severally became filled were consequently ascertained by direct experiment. In every instance they were found to require very different degrees of heat to become full, since in the same specimens some of the vacuities disappeared at 180° Fahr., others filled at temperatures slightly above that of boiling water, whilst many, though much reduced in size, remained perfectly visible at 365° Fahr.

Alluvial Deposits.—Although a very large amount of the gold annually obtained was no doubt originally derived from auriferous veins, not more than about one-third of the precious metal collected is procured directly from that source. The larger proportion of the gold now brought into the market is derived from alluvial diggings, in which it is separated by washing from the clay, sand, and gravel with which it is associated.

This gold-bearing drift belongs to at least two distinct geological epochs, both comparatively modern—although the latter period is distinctly separated from the earlier, its materials being chiefly derived from the disintegration and redistribution of the older deposits.

In California the more ancient deposits or "deep placers" are referable to a river-system different from that which now exists, flowing at a higher level, and frequently nearly at right angles to the direction of the main valleys of the present period.

The deep placers are in many localities covered by a thick capping of lava; and the eruptive matter covering them often occurs in the form of basaltic columns, beneath which are found the layers of sand, gravel, and boulders with which the gold is associated. The wood which occurs in these gravel-beds is either beautifully silicified, or is replaced by iron pyrites.

In the more clayey strata of these deposits leaf beds and impressions of leaves are not unfrequently found; and an examination of these made by Dr. Newberry authorizes the conclusion that the auriferous deposits lying beneath the lava are of tertiary age, and that they probably belong to the later Pliocene epoch. Water-worn gold is disseminated throughout the whole mass of these deposits, not, however, with uniformity, but always in greater abundance near the bottom, and more particularly in direct contact with the "bed rock," which is invariably grooved and worn by the action of water.

The materials of which these deep placers are composed are frequently consolidated into a sort of hard concrete, by being firmly bound together by crystalline iron pyrites; and sometimes this cementing material consists either of carbonate of lime or silica. The silica is rarely met with in a crystalline form; but near Kenebeck Hill a cavity, resulting from the junction of several pebbles, was found completely lined with well-defined crystals of quartz. These did not show, under the microscope, the usual fluid-cavities of quartz of the ordinary quartz veins of the country.

Where the cementing material of the conglomerate chiefly consists of pyrites, the enclosed trunks of trees are usually replaced

by that mineral, although of two pieces of wood lying in close proximity to each other, one may have become silicified, whilst the other is replaced by iron pyrites.

The assay of several specimens of the cementing pyrites showed that it invariably contained a certain but very variable amount of gold. In order to ascertain whether this exists in the form of water-worn grains mechanically enclosed within the sulphid, or in the form of spongy, crystalline, and filamentary particles, similar to those met with in the pyrites of auriferous veins, various samples were dissolved in nitric acid, and the the residues afterward subjected to microscopical examination. In this way granules of the precious metal, which had evidently been worn by the action of water, were detected, whilst others appeared not to have been subjected to such attrition. Mr. Ulrich states that in the gold-drifts of Australia, pyrites is often found replacing roots and drift-wood, and that samples have, on assay, yielded from a few penny-weights to several ounces of gold per ton.

Hot Springs.—Hot and boiling springs are exceedingly numerous throughout California; and considerable accumulations of sulphur, together with evidences of extensive solfatara action, are met with in different sections of the State.

The most remarkable instance on the Pacific coast of the actual growth, on a large scale, and at the present time, of mineral veins, is probably that afforded by the boiling springs in Steamboat Valley, about seven miles northwest of the great Comstock silver vein in the State of Nevada.

These springs are situated at a height of about 5000 feet above the level of the sea, at the foot of the eastern declivity of the Sierra Nevada. The rock in this locality presents several straight and parallel fissures, either giving out heated water or simply ejecting steam. The first group of crevices comprises five longitudinal springs extending in a straight line, and parallel to each other, for a distance of above 3000 feet. These fissures are partially filled by a siliceous incrustation, which is being constantly deposited on the sides, whilst a longitudinal central crevice allows of the escape of boiling water or steam. On the most eastern of these lines of fracture are five active centers of eruption, from which boiling water is sometimes ejected by the force of steam to a height of from eight to ten feet. These waters are alkaline, and contain, in addition to carbonate of soda, the sulphate of that base, together with chlorid of sodium. There is also everywhere an escape of carbonic acid, whilst from some places sulphuretted hydrogen is also evolved. These products, on arriving at the surface, give rise to the deposition of sulphur, silica, and anhydrous oxyd of iron. The silica and oxyd of iron form semi-crystalline bands parallel with the walls of the fissures; and spongy deposits accumulate around some of the points of most active emergence.

At a considerable distance to the west of those above described, a fissure having the same origin is observed; but this is no longer

traversed by currents of hot water, although it still gives off steam and carbonic acid at various points throughout its extent. At its northern extremity a central fissure still remains open; but in other localities it is, for the most part, obstructed by siliceous concretions. This siliceous rock is metalliferous, and, in addition to oxyd of iron and manganese, contains iron and copper pyrites. M. Laur states that he also discovered metallic gold in this deposit.

The rock enclosing the veins of Steamboat springs is granite, which in their vicinity is much decomposed, being often reduced to a cavernous skeleton of silica containing a few scales of mica.

Alkaline Lakes.—In that portion of California lying on the east of the Sierra Nevada are Mono Lake and Owen's Lake, both considerable sheets of water, highly impregnated with alkaline salts. Owen's Lake lies in lat. 36° 20″ south, long. 118° west from Greenwich, and is about twenty miles in length and eight in width.

The waters of this lake have a specific gravity of 1·076, and contain 7128·24 grs. of solid matter per gallon. The salts held in solution are chiefly carbonate and sulphate of soda, with chlorid of sodium; but potash, silica, and phosphoric acid are also present.

The incrustations, which at certain seasons of the year are found to the extent of many hundreds of tons, consist of a white spongy efflorescence, and are, as will be seen from the results of the analysis given in the paper, chiefly composed of carbonate of soda, mixed with a little chlorid of sodium and sulphate of soda.

General deductions.—The author remarks that, in the present state of our knowledge, the results of a careful examination of the gold-regions of the Pacific coast would appear to lead to the following conclusions:—

a. Quartz veins have generally been produced by the slow deposition from aqueous solutions of silica on the surfaces of the enclosing fissures.

b. From the general parallelism with its walls of the planes of any fragments of the enclosing rock which may have become imbedded in a vein, it is to be inferred that they were mechanically removed by the growth of the several layers to which they adhered, and that a subsequent deposition of quartz took place between them and the rock from which they had become detached. In this way were introduced the masses of rock known as "horses"

c. The formation of quartz veins is due to hydrothermal agencies, of which evidences are still to be found in the hot springs and recent metalliferous veins met with in various parts of the Pacific coast.

d. From the variable temperatures at which the vacuities in their fluid-cavities become filled, it may be inferred that they are the result of an intermittent action, and that the fissures were sometimes traversed by currents of hot water, whilst at others they gave off aqueous vapor or gaseous exhalations. This is pre-

cisely what is now taking place at Steamboat springs, where the formation of a vein is in progress, and from which currents of boiling water are often poured forth, whilst at other times the fissures give off currents of steam and heated gases only.

e. That gold may be deposited from the same solutions which give rise to the formation of the enclosing quartz, appears evident from the presence of that metal in pyrites enclosed in siliceous incrustations, as well as from the fact of large quantities of gold having been found in the interior of the stems of trees, which in deep diggings are often converted into pyrites.

f. The constant presence of iron pyrites in auriferous veins, and when so occuring its invariably containing a certain amount of gold, suggests the probability of this sulphid being in some way necessarily connected with the solvent by which the precious metal was held in solution. It has been shown that finely divided gold is soluble in the sesquichlorid of iron and, more sparingly, in the sesquisulphate of that metal. It is also well known that iron pyrites sometimes results from the action of reducing agents on the sulphates of that metal. If therefore sulphate of iron, in a solution containing gold, should become transformed by the action of a reducing agent into pyrites, the gold at the same time being reduced to the metallic state, would probably be found enclosed in the resulting crystals of that mineral.

g. The silica and other substances forming the cementing material of the ancient auriferous river-beds have probably been slowly deposited at a low temperature.

The connection existing between the decomposition of granite by the agency of boiling springs, the existence of alkaline plains, and the formation of lakes containing various salts of soda and potash, is too obvious to require comment.

4. *Gold in Rhinebeck, Dutchess county, New York.*—Reports on the auriferous quartz veins of Rhinebeck, have recently been published by Prof. J. G. Pohlé and Prof. John Torrey. According to the former, there are four veins dividing the talco-argillaceous schist in the direction of the cleavage. The quartz is cellular, and brown, with oxyd of iron arising from the decomposition of pyrites; but below it is more compact, and the pyrites is unaltered. Analysis of the rock afforded Prof. Pohlé 963 to 975 thousandths of gold, the rest almost solely silver. Prof. Pohlé adds: "The geological formations on this property are of the Lower Palæozoic age; the Potsdam periods of it are well represented by the metamorphic slates and the calciferous sandstone, but no fossils have thus far been found. The slates (argillaceous mainly) form a characteristic feature of the whole Appalachian range, and the presence of gold here, supplies the link heretofore wanting, in the chain of the Appalachian gold fields; for there can be no doubt that this is a part of the grand belt, taking its rise on the St. Lawrence river and scattering its auriferous treasure for thirteen hundred miles, along the southeastern border of North America, terminating in Alabama.

The discovery of gold in this part of the Appalachian gold fields is due to the scientific attainments and perseverance of E. G. Freligh, M.D."

Dr. Torrey assayed a quantity of the quartz procured by him at the mine, it being "a fair average of the lode," and amounting to several hundred pounds, and states as his results, that of three assays the average yield of gold per ton of ore (2000 lbs.) was $118.49. In no case was the proportion of gold less than $100 per ton.

III. BOTANY.

1. *Botanical Necrology for* 1868.—The most distinguished name upon the list is that of

George A. Walker-Arnott, Professor of Botany in the University at Glasgow, who died on the 17th of June last, in the 70th year of his age. He was born in Edinburgh, Feb. 6, 1799, educated at the celebrated High School of that city, and at the University, where he took high rank as a scholar, especially in the mathematics,—publishing two papers in Tilloch's Philosophical Magazine in 1817 and 1818, while yet a student in arts—and then, turning to law studies, he was called to the bar, as a member of the Faculty of Advocates in the year 1821. He hardly entered, however, upon the duties of his profession, his taste for natural history having been early developed under the lectures of Prof. Jameson and of Mr. Stewart,—the latter a well-known teacher of botany at that time, and his patrimonial estate of Arlary in Kinross-shire sufficing for his support, so that he could devote himself to botany, as he did, with unsurpassed ardor and success. His earliest botanical paper, upon some Brazilian Mosses, was written in French and published in a journal at Paris, in 1823. In 1826 and 1827 he contributed to the Edinburgh New Philosophical Journal a lively narrative of a botanical tour to the South of France and the Pyrenees. He resided for some time at Montpellier, and in Paris, examining the principal herbaria there, also that of DeCandolle at Geneva, and in 1828 the herbaria at St. Petersburgh. In 1831 he married and established himself with his collections at Arlary, where he resided until in 1845, he accepted the professorship of Botany in the University of Glasgow. It was during these fourteen years that the vast amount of scientific work he was able to accomplish was mainly done. He wrote the article *Botany* in the seventh edition of the Encyclopedia Brittanica,—the best treatise of the kind of its day in the English language, and one of the most influential. In conjunction with his early friend, Sir Wm. Hooker, he wrote the Contributions to the Flora of South America, &c., which form a long series of articles in the Botanical Miscellany, Journal of Botany and other similar periodical or serial publications edited by Sir William. He took a similar part in the Botany of Beechey's Voyage; in connection with Dr. Wight he brought out the first volume of the Prodromus Floræ Peninsulæ Indiæ Orientalis; and made numerous contributions to various periodicals.

Up to 1845 or somewhat later Dr. Arnott was one of the foremost botanists of the time, one of the most zealous and sagacious, versed alike in European and exotic botany. But upon assuming the duties of his chair at Glasgow he appears soon to have abandoned the field in which he had won the highest honors, and in which much more was justly expected. He assumed, however, the joint authorship of Hooker's British Flora, taking, we believe, the whole charge and responsibility of the later editions. As he began with Mosses, so for the last fifteen or twenty years of his life, he devoted himself principally to the *Diatomaceæ*, bringing to their investigation all the ardor of his nature and the keenest powers of observation combined with indomitable patience and unwearied care. So that he became in this department of microscopical research one of the highest authorities, and amassed one of the richest collections extant. As a professor he was greatly esteemed and respected, although he may be thought to have come almost too late in life to the professor's chair. In his later years he was much withdrawn from general botanical intercourse; but his surviving correspondents and friends on this side of the ocean cherish very pleasant memories of him.

Nathaniel Bagshaw Ward, Fellow of the Royal and Linnæan Societies, after whom, as its inventor, the Wardian case is named, died, at the ripe age of 77 years, on the 4th of June last. He was born in the east end of London, where his father was a medical practitioner of repute, and where for the greater part of his busy and most useful life he laboriously devoted himself to the same profession. About twenty years ago he exchanged the smoke-charged atmosphere and dingy dwellings of Wellclose Square for the pleasant and airy suburb of Clapham Rise, but still actively engaged almost to the last in professional practice and in his various official duties, mainly in connection with the Apothecaries' Society, filling in succession nearly all its important offices. The renovation and even the maintenance of the celebrated Apothecaries' Garden at Chelsea—the oldest botanical establishment of the country—is probably mainly due to his counsels and exertions. We cannot here enter into the interesting history of the invention of the now familiar Wardian case,—a discovery which grew out of Mr. Ward's persistent endeavors to cultivate the plants he delighted in under the smoke and soot of the dingiest part of London, and which resulted in providing for the poor as well as the rich denizens of the smoky towns of the Old World the inexpensive but invaluable luxury or comfort of being surrounded at all seasons with growing plants and fresh flowers. Nor is the invention less applicable to house-culture, especially of Ferns, under the clearer and purer air of our own country rendered arid by the cold of winter, as hundreds could testify who have enjoyed the benefit, perhaps without knowing even the name of their benefactor. Equally important is the application of the Wardian case to the conveyance of living plants between distant countries. The writer well remembers the first case of growing

plants sent to New York thirty-five years ago, which arrived as fresh and healthy as when they left London; and the transmission was quite as successful between England and Australia, when the voyage, confined to sailing ships, was far longer than now. So useful has this contrivance proved to be in this respect, that the Director of Kew Gardens "feels safe in saying that a large proportion of the most valuable economic and other tropical plants now cultivated in England would, but for these cases, not yet have been introduced." The earliest published account of the Wardian case was given by Mr. Ward in the form of a letter to his near friend, the late Sir William Hooker, and was printed in the Companion to the Botanical Magazine for May, 1836. His volume, "On the Growth of Plants in closely-glazed Cases" appeared in the year 1842, and a second edition, considerably enlarged and suitably illustrated, was published a few years later. These were, we believe, Mr. Ward's only scientific publications, excepting reports of communications to various societies with which he was connected, several of them relating to a subject near to his heart, the improvement of the dwellings of the poor in England, and the amelioration in other respects, of their hard condition. A most enthusiastic and in some departments a learned botanist, his contributions to his favorite avocation were not in the form of authorship, to which he seemed averse: a man "given to hospitality" indeed, but as unpretending as it was cordial and unlimited. The coming generation will hardly appreciate the extent of the influence he exerted and the strength of the attachment he inspired so widely among the cultivators of natural science, nor understand, perhaps, how it could be said of him, and without exaggeration, that "for very many years his hospitable house, first in Wellclose Square, and latterly at Clapham Rise, was the most frequented metropolitan resort of naturalists from all quarters of the globe of any since Sir Joseph Banks' day." But while any survive of those who have had the privilege of knowing him personally, or in the friendly correspondence he delighted in, Mr. Ward will be remembered as "one of the gentlest, kindest, and purest," and in the highest sense one of the best of men.

Edward Pœppig, Professor of Zoology in the University of Leipsic, died on the 4th of September, at the age of 70 years. His name is familiar to botanists on account of the collections he made in Cuba, Chili, Peru, and the upper part of the Amazon; which were in part published in Pœppig and Endlicher's Nova Genera et Species Plantarum, etc. Soon after his return from his long journey of exploration, he took the chair of zoology at Leipsic, and consequently abandoned botany.

We hear of the decease of Dr. Schnitzlein in Germany, and of the venerable François Delessert, the head of the house of Delessert since the death of his brother Benjamin, the life-long friend of DeCandolle, and the possessor of the botanical museum and rich library so well known to and so freely at the service of botanists.

A. G.

To these names must be added that of HORACE MANN, assistant in the "Gray Herbarium" at Cambridge. When Dr. Gray sailed for Europe, in September last, Mr. Mann was in failing health, and probably the above necrological notices were written by Dr. Gray about the time of his assistant's death, which took place Nov. 11.

Mr. Mann's botanical fame will rest chiefly upon the large collections which he made in the Sandwich Islands in connection with Mr. W. T. Brigham, and upon the subsequent "Enumeration of Hawaiian Plants" published in the Proceedings of the American Acad. of Arts and Sciences, for Sept. 11, 1866.

In botany itself his name is happily commemorated by the striking genus *Hesperomannia* (Gray), a Mutisiaceous Composite which he discovered in the little island of Lanai of the Hawaiian group. D. C. E.

2. *Index to the Native and Scientific names of Indian and other Eastern Economic Plants and Objects, originally prepared under the authority of the Secretary of State for India*, by J. FORBES WATSON, M.D., etc., etc. London: Trübner & Co. 1868. —A large, beautifully printed volume, of 637 oblong 4to pages, in double columns, the native names belonging to 30 oriental and several European languages, followed by the recognized scientific name, embodying the result of much erudition and immense labor. It will be useful to botanists on the one hand, to philologists on the other, and not less so to commercial men who have occasion to seek for information concerning Indian and other eastern plants and vegetable products, information otherwise oftentimes very difficult to find. A. G.

3. *Synopsis Filicum; or a Synopsis of all known Ferns, including the Osmundaceæ, Schizæaceæ, Marattiaceæ and Ophioglossaceæ (chiefly derived from the Kew Herbarium)*. Accompanied by figures representing the essential characters of each genus; by the late Sir WILLIAM JACKSON HOOKER, K.H., D.C.L., F.R.S., A.S., and L.S., Director of the Royal Gardens at Kew, and JOHN GILBERT BAKER, F.L.S., Assistant Curator of the Kew Herbarium. London: Robert Hardwicke, 1865–1868. 8vo, pp. 482, tt. 9.— With the concluding part of the "Species Filicum" its author announced that he was preparing, should life and health be spared to accomplish it, this Synopsis, to be a supplementary volume to the work then just completed, reviewing all the recognized sections, genera and species, giving them brief characters, with general habitats, and with references for synonyms, more full localities, figures, etc., to the pages of the "Species," adding all needful corrections and recently discovered species, and finally embracing the sub-orders named above. The full accomplishment of this purpose he was not permitted to see, for "whilst the sheet which terminates at page 48 was passing through the press, Sir W. Hooker's long career of botanical authorship was somewhat unexpectedly terminated by his death." The manuscript notes already prepared and the annotated copy of the "Species Filicum" were entrusted by Dr. Hooker to Mr. J. G. Baker, an ardent Botanist, already

well known for his volume on the "Botany, Geology, Climate, and Physical Geography of North Yorkshire," and for his "Supplement to Baines' Flora of Yorkshire," with the request that the Synopsis should be carried out to its completion in strict accordance with the original plan.

Mr. Baker has performed his task faithfully and well, and with commendable diligence, for he has thoroughly studied the Ferns, has made himself familiar with the plan of the work already begun, and has brought it to a well-ordered conclusion, all within two years after the death of his venerable predecessor in pteridological science. This book is now the only one which gives a complete view of all known Ferns, and although almost any other writer on the subject would have admitted a considerably larger number of species, it will be to all fern-lovers a most useful work, and in accordance with its classification and nomenclature most collections of Ferns will conveniently be arranged. The number of species admitted and described is 2235. But very many names of doubtfully distinct or imperfectly known species are given, with a few words of diagnosis, along with the recognized species with which they are most closely allied. Of the genera not reached in the "Species Filicum" it may be well to note in this place that *Osmunda* has 6 admitted species, *Todea* has 4, *Schizœa* 6, *Aneimia*, or as it is here written with Swartz' own mistaken orthography, *Anemia*, 26, *Mohria* 1, *Trochopteris* 1, *Lygodium* 16, *Angiopteris* 1, *Marattia* 7, *Dancœa* 11, *Kaulfussia* 1, *Ophioglossum* 9, six of which it is quite possible that Sir W. J. Hooker would have united with *O. vulgatum*, *Helminthostachys* 1, and *Botrychium* 6. Of the species familiar to N. American botanists the *Osmundas*, our *Schizœa*, our *Lygodium* and our *Ophioglossum* are left as we know them, *Botrychium simplex* is admitted, *B. lanceolatum* regarded, and probably with justice, as a variety of *B. rutaceum*, *B. lunarioides* is referred to the earlier known *B. ternatnm*, as had already been done by Milde, and *B. Virginianum* is made to include the East Indian *B. lanuginosum*, commonly a larger and more hairy plant. The name is here *virginianum* as written by Swartz, and before him by Linnæus. The name *Virginicum*, commonly received in N. America, seems to have originated with Michaux, from whom it was copied first by Willdenow, and then by all N. American botanists to this day. A few pages of "addenda et corrigenda," and a full index of species and synonyms conclude the work. D. C. E.

.4. *Plantæ Wrightianæ Cubenses.*—Mr. Wright's last distribution of his Cuban collections was made in 1865, or late in 1864. Since then he has been exploring the botany of western Cuba, and has again made very large collections, many of the species being new to the series, for the numbers have ascended from about 3,500 to 3,963. The distribution into sets is now made, all the former sets which remained unsold being incorporated into the new distribution, so that the fullest set contains 2,253 species, and the thirteenth has 637. The larger sets must contain by far the greater

part of the flora of Cuba, and the smaller ones will be of great value to amateur botanists in showing the character of tropical plants. A catalogue of names will be published in due time, especially since it is understood that Mr. Wright has now in preparation his "opus magnum" of a Flora Cubensis.

Persons wishing to purchase these plants may address Charles Wright, Esq., Wethersfield, Conn., or Prof. D. C. Eaton, New Haven, Conn. The price per century is fixed at the low rate of ten dollars in currency. D. C. E.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Flow of the Great Lakes. Interesting Experiments.*—The Detroit *Post*, in an article on the various methods that have been tried from time to time to solve the mystery of the supply and outflow of the Great Lakes, gives an account of a new and successful apparatus just completed, for the purpose of measuring accurately the velocity of the currents in their tidal flow into and out of the lakes. The *Post* says:

"It is now two years since the newspapers of the West began to discuss whether the great lakes are fed by subaqueous springs, or have hidden outlets. The party who favored the theory of subaqueous springs asserted that more water flowed out the St. Lawrence than could be poured in by all the sources of supply known to exist, while the upholders of the idea of hidden outlets contended that evaporation and the visible outflow could not account for all the water which the lakes received and distributed.

"General W. F. Reynolds, Superintendent of the Lake Survey, determined to give this subject such consideration as, in the West, could only be afforded by the engineers employed on that work, and, accordingly, for the past two summers, observations have been made in the St. Mary's, St. Clair, Detroit, Niagara and St. Lawrence rivers for the purpose of ascertaining the exact amount of outflow of the lakes. The river-guaging has, from the start, been entrusted to Assistant D. Farrand Henry, of Detroit, and the apparatus used is one of his own invention. This apparatus is so much more delicate and accurate than any previously tried that the results are of great value.

"To calculate the amount of outflow of any stream, it is necessary to have the area of the body of water, and its mean velocity, at any point. These two quantities multiplied together give the discharge. The first is easily obtained by making frequent soundings across the stream on a known line. The second is more difficult. The only practical methods heretofore in use, for the determination of the velocity are, first by the time of passage of floats past a known line; second, by the difference in the height at which water will stand in two tubes, one of which is bent toward the current at the bottom and the other is straight; and, third, by water mills, as they are termed, which consist of float wheels exposed to the current, the number of revolutions being recorded by

a system of decimal gears or telltale. Of these methods the first is the only one which has been used in deep water."

Mr. Henry was dissatisfied with these methods, and devised a "Telegraphic Current Meter," which he has used with perfect success during the past season.

"This meter consists of a propeller, or float wheel, which has on its hub an eccentric, and on the axle an ivory lever, which has one end kept on the eccentric by a light spring, while into the other end a hole is drilled, meeting another hole, drilled at an angle with it, near the center of the bottom side. Into these holes a platinum wire is forced, so that the lever rests on the point of the wire coming out of the center hole. Under this point a small platinum plate is fastened to the axle. The other end of the wire is connected by a hinge joint to a long copper wire, which is fastened to the axle, but insulated from it. At the rear end of the axle are two vanes, at right angles to each other, sufficiently large to keep the wheel in the thread of the current. The whole is suspended by a yoke which has two small eyes on its side.

"The method of using the meter is as follows: A boat being anchored in the stream at the point where the current is to be tested, a weight with a copper wire attached is let down from the stern. The upper end of this wire is fastened to a spring pole, which takes up most of the motion of the boat. This wire is passed through the eyes on the side of the yoke in the meter, a measured cord is fastened to a swivel ring in the upper, and a weight to one in the lower end of the yoke. The meter may now be lowered to any depth, sliding down the anchored wire, the upper end of this wire and of that fastened to the platinum point, being connected with a battery in the boat; then at every revolution of the wheel the circuit will be opened and closed by the eccentric, raising the ivory lever, and thus breaking the connection between the platinum point and plate. If now a Morse's paper register be placed in the circuit, at every revolution of the wheel a dot will be made on the moving paper, and thus the number of revolutions in any given time can be ascertained.

"The observations in the rivers were taken on a known line, one hundred feet apart, and at each five feet of depth. One of the first things noticed was the irregularity of the beat of the counter, showing that the current pulsated.

"The pulsations are not regular, the common maximums being from one-half to one and a half minutes apart, with every five or ten minutes a greater increase or decrease. They are least in the maximum current, and increase toward the bottom and sides of the stream.

"The maximum velocity of the current was found to be at or a little below the surface, and the velocity at the bottom is probably not over two-thirds the maximum.

"The following approximate velocities and discharges of the different rivers is taken from the computations of the work last year. The quantities for the Detroit River are accurately computed:

RIVER.	Maximum velocity. Feet per second.	Maximum velocity. Miles per hour.	Mean velocity. Feet per second.	Mean velocity. Miles per hour.	Disch'ge cubic ft. per second.
St. Marie's	1·921	1·30	0·967	0·66	90,783
St. Clair	4·544	3·09	3·514	2·39	233,726
Detroit	4·800	2·71	3·000	2·04	236,000
Niagara	3·370	2·32	2·258	1·54	242,494
St. Lawrence	1·462	1·00	0·954	0·65	319,943

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *How Crops Grow. A Treatise on the Chemical Composition, Structure, and Life of the Plant, for all Students of Agriculture.* With numerous illustrations and tables of analyses; by SAMUEL W. JOHNSON, M.A., Professor of Analytical and Agricultural Chemistry, in the Sheffield Scientific School of Yale College. New York, [1868] Orange Judd & Co.—The appearance of Professor Johnson's volume is well timed. Every American teacher who has attempted hitherto to inform his students of the bearings of chemical or physiological science, upon the art and practice of agriculture has keenly felt the want of text-books fitted to enforce his precepts and make good his own omissions and defects. It has been plain that, in the lack of such books, few of the schools of agriculture which are now coming into operation in this country, could even attempt to teach their students in accordance with the hopes and demands of the communities in which they have been established. It is consequently a matter of no slight importance when an acknowledged master applies himself to supply the want.

The mere allusion to some of Professor Johnson's earlier essays, such as those on Soil Analyses,* and Agricultural Science,† will recall to the minds of many of our readers the pleasure and satisfaction with which those admirable papers were first received and will secure a hearty welcome for the new work. Before an audience so familiar with the learning and good sense of the author and with the clearness of his style, it would be useless to dwell upon these characteristics, or to say more of them than that they are all conspicuous in the present work. But there are other features peculiar to the book just published, which should be particularly mentioned. It is in the first place, a well-considered and thorough-going attempt to impart knowledge by the inductive and experimental method; as such, it supplies a deficiency that has long existed in English literature.

"For those who have not enjoyed the advantages of the schools, the author has sought to unfold his subjects by such regular and simple steps, that any one may easily master them. It has also been attempted to adapt the work in form and contents to the wants of the class-room by a strictly systematic arrangement of topics, and by division of the matter into convenient paragraphs."—p. 5.

But besides charging the mind of the student with valuable information in a thorough and logical way, the intention is so to impress upon him the manner and methods by which questions are

* This Journal, xxxii, 233. † Ibid., xxviii, 71.

asked of inanimate things, that he may himself become in some sort an investigator of truth.

In these days when a belief in the power of evolving all things from the depths of consciousness is so strong among men, there is something truly refreshing in the spirit of the following paragraphs. Strange as the assertion may seem, there is many a "modern" scientist who dares not enunciate some of these simple truths.

"The *art* of Agriculture consists in certain practices and operations which have gradually grown out of an observation and imitation of the best efforts of nature, or have been hit upon accidentally. The *science* of agriculture is the rational theory and exposition of the successful art."

"Strictly considered, the art and science of agriculture are of equal age, and have grown together from the earliest times. Those who first cultivated the soil by digging, planting, manuring and irrigating, had their sufficient reason for every step. In all cases, thought goes before work, and the intelligent workman always has a theory upon which his practice is planned. No farm was ever conducted without physiology, chemistry and physics, any more than an aqueduct or a railway was ever built without mathematics and mechanics. Every successful farmer is, to some extent a scientific man. Let him throw away the knowledge of facts and the knowledge of principles which constitute his science, and he has lost the elements of his success. The farmer without his reasons, his theory, his science, can have no plan; and these wanting, agriculture would be as complete a failure with him as it would be with a man of mere science, destitute of manual, financial and executive skill" (p. 17.) * * * * "It is the boast of some who affect to glory in the sufficiency of practice and decry theory, that the former is based upon experience, which is the only safe guide. But this is a one-sided view of the matter. Theory is also based upon experience, if it be truly scientific. The vagarizing of an ignorant and undisciplined mind is not theory. Theory in the good and proper sense, is always a deduction from facts, the best deduction of which the stock of facts in our possession admits. It is the interpretation of facts. It is the expression of the ideas which facts awaken when submitted to a fertile imagination and well balanced judgment. A scientific theory is intended for the nearest possible approach to the truth. Theory is confessedly imperfect, because our knowledge of facts is imcomplete, our mental insight weak and our judgment fallible. But the scientific theory which is formed by the contributions of a multitude of earnest thinkers and workers, among whom are likely to be the most gifted intellects and most skillful hands, is, in these days, to a great extent worthy of the divine truth in nature, of which it is the completest human conception and expression."

"Science employs, in effecting its progress, essentially the same methods that are used by merely practical men. Its success is commonly more rapid and brilliant, because its instruments of observation are finer and more skillfully handled; because it experiments more industriously and variedly, thus commanding a wider and more fruitful experience, because it usually brings a more cultivated imagination and a more disciplined judgment to bear upon its work."—(p. 22).

"It has been sought to present the subject inductively, to collate and compare, as far as possible, all the facts, and so to describe and discuss the methods of investigation that the conclusions given shall not rest on any individual authority, but that the student may be able to judge himself of their validity and importance. In many cases fulness of detail has been employed, from a conviction that an acquaintance with the sources of information and with the processes by which a problem is attacked and truth arrived at, is a necessary part of the education of those who are hereafter to be of service in the advancement of agriculture. * * * The subject should be taught so thoroughly that the learner may comprehend at once the deficiencies and the possibilities of our knowledge. Thus we may hope that a company of capable investigators may be raised up, from whose efforts the science and the art may receive new and continual impulses."—(p. iv).

Whether the hope expressed in the last sentence is likely to be realized in the near future is a debatable question which can only be answered by experience. In any event Professor Johnson is to be congratulated in having thus distinctly set forth a noble conception. He may take comfort also in the reflection that he has striven faithfully in behalf of the idea. It will be no fault of his if the times prove inauspicious. The aim is manifestly a high and legitimate aim. When the present rage for burdening the Colleges and Schools of Agriculture with experimental farms, conservatories of plants and collections of costly implements shall have gone by, as it has gone by in Europe, and the principles upon which agricultural art depends come to be taught in a rational and methodical way then the student will have time and temper for the mental discipline and sober reflection so essential for the acquirement of habits of thought and investigation. Then the seed now sown will doubtless bring forth good fruit. It will be strange indeed if the general acceptance of correct notions of teaching agriculture be not greatly hastened by the work before us.

The present volume, though complete in itself and printed as an independent work, is in reality but one of a series of volumes by the same author to be published hereafter. In the second volume now in press, entitled "How Crops Feed," the food of plants and the relations of the atmosphere and the soil to the plant, will be discussed in detail. A third volume, upon Cultivation; or the Improvement of the Soil and of Crops by Tillage and Manures, is promised at an early day; while a fourth work on Stock Feeding and Dairy Produce, considered from the point of view of chemical and physiological science, may finish the series.

It is a bright omen for the future of our country when in aid of a great educational revival such as now animates the land there are produced within its limits books so sensible, so judicious and so learned as this first volume of the promised series unquestionably is. It is a safe prediction that the reputation of the book will not be confined to American soil. F. H. S.

2. *Outlines of Comparative Anatomy and Medical Zoology;* by Harrison Allen, M.D. Philadelphia, J. B. Lippincott & Co. 8vo. 1869.—In the first part of this work the author has given a very concise compendium of the Comparative Anatomy of all classes of Animals. The definitions and explanations are usually clear and accurate, although brief, and the arrangement of subjects very convenient.

The second part is devoted to Medical Zoology, and contains detailed descriptions of all animals immediately connected with medical science, whether on account of their medicinal products, venomous powers, or parasitism. The portion relating to parasitic worms will be found particularly useful.

It is scarcely to be expected that a work like this and covering so wide a field would be entirely free from errors or imperfect statements, yet in this there are fewer instances of these than in most works upon the same subject. Most of those noted are of

minor importance: such as the statement on page 16 that *Alcyonium* has six tentacles (correctly stated on page 14, however); on page 25 the distinction between the "skeletons" of *Pennatula* and *Renilla*, and the statement that the valves of Brachiopoda are anterior and posterior; on page 106 where, apparently, the winter-buds (statoblasts) of Polyzoa are regarded as eggs. The division of Radiata into Echinodermata and Cœlenterata (p. 15), although sustained by good authorities, is objectionable, since the latter can be regarded only as an artificial group, at times convenient, perhaps, like the term Invertebrata.

As a whole the book is a very useful one, and will supply a want long felt in this country by teachers and students, as well as by physicians and others interested in Comparative Anatomy. In no other work upon the subject can so many important facts and clear explanations of structure be found in so small a compass. v.

3. *A List of the Birds of New England;* by ELLIOTT COUES, M.D. From Proceedings of the Essex Institute, Salem, Mass. Vol. v. 1868.—During several years past much attention has been given to the distribution of American birds by many ornithologists, and a number of local catalogues have been published. In the present work the author has given a complete and very useful summary of all the important information of this kind that we at present possess with reference to New England Birds. v.

4. *Synopsis of the Birds of South Carolina;* by ELLIOTT COUES, M.D. From the Proceedings of the Boston Society of Natural History. 1868.—An interesting contribution to our knowledge of the distribution of birds in the Southern States. The season of occurrence and relative abundance is given for each of the 294 species. v.

5. *The Butterflies of North America.* Part 2. By WM. H. EDWARDS. Philadelphia, August, 1868. 4to, with five colored plates.—In this number there are descriptions and beautiful illustrations of *Argynnis Callipe*, *A. Hesperis*, *Colias Alexandra*, *C. Helena*, *C. Christina*, *C. Behrii* and *Apatura Alicia*, the latter a new species from New Orleans. v.

6. *Extra Digits;* by BURT G. WILDER, M.D. From Publications of the Mass. Medical Society. Vol. ii, no. 3. Boston, 1868. —Many curious statistics concerning the occurrence of supernumerary fingers and toes, obtained from 152 cases, are given in this memoir, and their significance and interest well discussed. The author solicits additional information and appends a list of questions to be answered in recording new cases of Extra Digits. v.

7. *The Natural Wealth of California, &c.;* by TITUS TEY CRONISE. pp. 696. 1868. San Francisco: H. H. Bancroft & Co.—This volume, which has been some time on our table, is a digest of interesting facts relating to the early history, geography, topography and scenery; climate; agriculture and commercial products; geology, zoology and botany; mineralogy, mines and mining processes, internal communications, immigration, population, and educational institutions. Each County is separately treated. Less

statistical and dry than Hittell's work, Mr. Cronise has made his volume more interesting to the general reader. He has had the advantage of the coöperation of several persons well versed in different departments, as well as a long and intimate familiarity with the commercial and mining resources of the State. Now that the Pacific Railroad promises to place California within reach of a summer excursion, it is important that its resources and wonders should be more familiarly known than hitherto by people on the Atlantic border, and to this end the present volume is timely and well adapted.

8. *Outlines of Physiology, Human and Comparative;* by JOHN MARSHALL, F.R.S., Professor of Surgery in University College, London; Surgeon to the University College Hospital. With additions, by FRANCIS G. SMITH, M.D., Prof. of Institutes of Medicine in the University of Pennsylvania. 8vo, pp. 1026. 1868. Philadelphia: Henry C. Lea.—This is an elaborate and carefully prepared digest of human and comparative physiology, designed for the use of general readers, but more especially serviceable to the student of medicine. Its style is concise, clear and scholarly, its order perspicuous and exact, and its range of topics extended. The author and his American Editor have been careful to bring to the illustration of the subject, the important discoveries of modern science in the various cognate departments of investigation. This is especially visible in the variety of interesting information derived from the departments of chemistry and physics. The great amount and variety of matter contained in the work, is strikingly illustrated by turning over the copious Index, covering twenty-four closely printed pages in double columns.

9. *Reliquiæ Aquitanicæ.*—(H. Baillière, London; Baillière Brothers, New York).—Part v, of this handsomely illustrated work, in quarto, on the relics of man and of associated animals in the caves of Southern France, continues the account of the geology of the Vezère, and contains also a chapter "on the similarity of some implements found in the caves of Dordogne, to some used by the North American Indians," together with observations on the "Germani" of the Roman period, and on the range of the Reindeer. Beside plates of stone arrow-heads and chippings, there are views of the region of the Vezère in Dordogne.

10. *Geological Survey of Illinois;* A. H. WORTHEN, Director. Vol. III, Geology and Palæontology, v, and 574 pp. large 8vo, with map and sketches, and 20 plates of fossils. 1868. Published by the authority of the Legislature of Illinois.—This volume of the Geological Survey of Illinois has just been issued by the Director, Prof. Worthen, and deserves a much more extended notice than our present space will permit. Part I. (pp. 1 to 287) is devoted to Geology, and in the opening chapter Prof. Worthen gives an able exposition of the Coal measures and Carboniferous system of Illinois, and their relations to the same formation in some of the neighboring States. The subsequent chapters, by Prof. Worthen and his field-assistants, Messrs. Engelmann, Free-

man and Bannister, relate more especially to the geology of various counties of the State, and contain much interesting local and general information.

Part II, (pp. 291–574,) is a valuable contribution to American Paleontology, and reflects much credit on its authors, Messrs F. B. Meek, and the Director of the survey. Many new species of paleozoic fossils are here described, and among them, the interesting Articulates from the Coal-measures will especially attract the attention of paleontologists. An important Supplement, by Mr. S. H. Scudder, containing descriptions of fossil insects from the Coal measures of the State, and another by Dr. J. V. Z. Blaney, giving the results of chemical analyses of various limestones, complete the work, which forms a worthy companion volume to those already issued.

PROCEEDINGS ACAD. NAT. SCI., PHILADELPHIA, No. 2, March, April, May, 1868.—p. 92, Remarks on fragments of a large Enaliosaurian; *E. D. Cope.*—p. 93, Remarks on the New Species of Osmerus; *T. Norris.*—p. 94, Unionidæ from Lake Nicaragua; *I. Lea.*—p. 96, Reptilia, and Batrachia obtained by the Orton Expedition to Equador and the upper Amazon, with notes on other species, and a Second Supplement on some New Raniformia of the Old World; *E. D. Cope.*—p. 140, Sexual Law in Acerdesycarpum; *T. Meehan.*—p. 142, On a New Mineral in Cryolite; *T. D. Rand.*—p. 143, Description of Sixteen New Species of Unio of the United States; *I. Lea.*—p. 145, Notes on some Singular Forms of Chinese species of Unio; *I. Lea.*—p. 147, Remains of a New Ophidian from the Green Sand of New Jersey; *E. D. Cope.*—p. 148, List of Birds collected at Laredo, Texas; *H. B. Butcher.*—p. 150, Descriptions of four New Species of Exotic Unionidiæ; *I. Lea.*—p. 151, Description of Twenty-six New Species of Melanidæ of the United States; *I. Lea.*

PROCEEDINGS BOSTON SOC. NAT. HIST. Vol. XII.—p. 81, Description and History of a New Species of Erirhinus; *F. G. Sanborn.*—p. 82, Eruption of Mauna Loa; *W. T. Brigham.*—p. 83, Projection of After-pictures; *B. J. Jeffries.*—p. 84, Analysis of Petrosilex; *C. T. Jackson*—p 85, Birds of Iowa and Illinois; *J. Allen.*—p. 86, The Hairy Men of Yesso; *W. P. Blake.*—p. 87, Migratory Grasshoppers of the United States; *S. H. Scudder.*—p. 88, Apatite at Perth, Canada West; *C. T. Jackson.*—p 90, Larva and Pupa-case of Microdon globosus; *F. G. Sanborn.*—p. 91, Comparison of Young Larvæ of Insects; *L. Trouvelot.*—p. 92, Analogy between Lima-codes and some Hymenoptera; *L Trouvelot*—p. 94, The Thaumatrope; *B. J. Jeffries.*—p. 96, New Mineral Locality, Auburn, Me.; *L. Hills.*—p. 97, Metamorphosis of Siredon into Amblystoma; *O. C. Marsh.*—p. 99, Reproduction of lost limbs in the Walking-stick; *S. H. Scudder.*—p. 100, On a Thread Worm infesting the Brain of the Snake-bird; *J. Wyman.*—p. 104, Synopsis of the Birds of South Carolina; *E. Coues.*—p. 128, On the Nature of the Movements involved in the Changes of Level of Shore Lines; *N. S. Shaler.*—p. 136, Deep Dredging between Cuba and Florida; *Pourtales.*—Disappearance of the Cane from the central part of the Ohio valley; *N. S. Shaler.*—p. 138, Nephrite from Turkistan, *R. von Schlagintweit.*—p. 139, Hodotermes Japonicus; *H. Hagen*—A Century of Orthoptera, Decade I, Gryllides; *S. H. Scudder.*—p. 143, Natural History of Alaska; *W. H. Dall.*—p. 145, Absence of Glacial Action in the valley of Youkon River; *N. S. Shaler.*—p. 152, Obituary of Horace Mann; *W. T. Brigham.*—p. 157, Nautilus pompilius in Alcohol, from the Moluccas; *A. S. Bickmore.*—p. 158, Results of Mann's study of the Hawaiian Flora; *W. T. Brigham.*